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— Xuf.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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NUMBER CXXVII.—SUPPLEMENT TO VOL. XIX.

PLATE.

I. Illustrative of Dr. DRAPER's Paper on some Analogies between the Phænomena of the Chemical Rays, and those of Radiant Heat.

ERRATA.

Page 261, line 20, *for* an atomic element *read* a chemical equivalent.
Page 265, line 10, *for* $= q\ V$ *read* $= q\ v$.
Page 271, line 9, *for* twenty-four inch, *read* 20 four-inch.
Page 273, line 2, *dele* Fig. 6.
Page 320, line 2 from the bottom, for *antè* read vol. iii.
Page 327, line 13, *for* 24″ *read* 54″.

The notices of papers read before the Royal Society referred to in vol. xviii. p. 547 note, will be found in the present volume, p. 240.

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[THIRD SERIES.]

JULY 1841.

I. *On the Spontaneous Evolution of Sulphuretted Hydrogen in the Waters of the Western Coast of Africa, and of other Localities.* By J. FREDERIC DANIELL, *For. Sec. R.S., Prof. Chem. in King's Coll., London, &c.**

MY attention was first directed to the subject, which I shall have the honour of submitting to your notice this evening, by the Lords of the Admiralty, in April 1840, who sent me eight specimens, and afterwards two additional specimens, of water from the mouths of the rivers on the western coast of Africa, with directions to analyze them, for the purpose of discovering, if possible, the cause of the rapid decay of the copper-sheathing of ships employed upon those stations.

Of the comparative duration of the metal in the vessels of the Royal Navy I have not been informed; but the evil complained of is well known also in the merchant service; and upon inquiry of one of the largest copper-smelters in South Wales, he assures me, that "the experience of between thirty and forty years has led his mind to the conclusion that sheathing copper will be as much or more injured on a nine months' voyage to and along the coast of Africa, as by the wear of from three to four years on any other trade."

The first water which I examined was from the river at Sierra Leone, taken at three miles from the mouth. Upon drawing the cork of the bottle it was found to smell very strongly of sulphuretted hydrogen.

* Communicated by the Author; being the substance of a Lecture delivered to the Members of the Royal Institution on the 21st May, 1841.

The first idea which occurred to me was that which seems generally to have prevailed upon similar occasions, viz. that this gas was generated from some change which had taken place in the water after it had been bottled, from the decomposition of some animal or vegetable substance; but a little consideration showed that this explanation was quite inadmissible, inasmuch as the sediment from the whole bottle did not exceed half a grain, the water being perfectly bright, and the salts upon evaporation *snow-white*; and the water became perfectly sweet a very short time after it had been exposed to the air.

Indeed the common prejudice regarding the unlimited quantity of sulphuretted hydrogen generated by putrescence is perfectly untenable, and is founded solely upon its disagreeable odour. The fact is, that the quantity of sulphur in animal matter is very small, and the nauseous smell is by no means an infallible criterion of the existence of the gas.

As a natural product, sulphuretted hydrogen has hitherto been known chiefly as an ingredient in certain mineral waters, such as those of Harrowgate and Aix la Chapelle; the former of which contains, per gallon, 18·4 cub. in., the latter 44·0. The comparatively small springs which yield these waters are most carefully preserved by their proprietors, on account of their medicinal virtues, and the profits which are derived from their use.

The generation of the gas with which such waters are impregnated, has been usually attributed to some unknown action upon pyrites and other sulphurets of the metals in the interior of the earth, and it is pretty generally ascribed to volcanic action. It has also been generally known that sulphuretted hydrogen is produced by processes of decay or fermentation, in which large quantities of animal matters are concerned.

To the natural sources of this gas must now be added the estuaries of many large rivers and immense tracts of the Ocean in their immediate vicinity.

The results of the analysis of the African waters, sent to me from the Admiralty, embracing an extent of fifteen or sixteen degrees of latitude, are as follows:—

Each bottle was properly corked and sealed, and contained about three imperial pints; the water in all was perfectly bright, and had deposited very little sediment.

The first which I examined was labelled, "Water from the river at Sierra Leone, taken at three miles from the mouth, by Her Majesty's brigantine *Dolphin*, at low water, spring tides, on the 24th day of September, 1839, during the rainy season. (Signed) "Edward Holland, *Lieut.-Com.*"

Upon drawing the cork of this bottle, it was found to smell very strongly of sulphuretted hydrogen. The sediment in the bottle only amounted to 0·5 grain of vegetable matter. Specific gravity to 1018·5. The results of the analysis, calculated for the imperial gallon:

Sulphuretted hydrogen	6·18 cub. in.	
Chlorine	943·14 grs.	
Sulphuric acid	82·70	
Lime	19·14	
Magnesia	27·68	
Magnesium	32·71	
Sodium	563·33	
		1668·70

There was also a trace of potassa in this water.

The actual amount of dry salts obtained by evaporation, was 1696·0 grains. The difference between this and the results of the analysis is not more than usual in similar cases, and arises from the impossibility of determining the exact mode in which the several acids and bases are combined in the water, and from the difficulty of drying the salts without the decomposition of a portion, at least, of some of them.

2. "Water from the river Volta, taken [at sea] twenty-eight miles from the mouth, bearing W.N.W., by Her Majesty's schooner *Fair Rosamond*, latitude 5° 37′ north, longitude 1° 10′ east, on the 4th of September, 1839; season not rainy."

This water also smelt very strongly of sulphuretted hydrogen; the sediment in the bottle did not exceed 0·3 grain of vegetable matter. It contained, per gallon,

Sulphuretted hydrogen	6·99 cub. in.
Chlorine	1411·68 grs.
Sulphuric acid	92·47
Lime	14·75
Magnesia	35·70
Magnesium	12·46
Sodium	916·20
Potassium	a trace
	2483·26

Specific gravity, 1025·4

Amount of salts from evaporation . 2480·0

3. "Water from the river Bonny, taken at anchor off the

town in the river, by Her Majesty's schooner *Fair Rosamond*, on the 9th of October, 1839, about the conclusion of the rainy season."

This water smelt slightly of sulphuretted hydrogen, and the sediment in the bottle weighed only 0·4 grains, and consisted of vegetable matter. The results of the analysis were, per gallon,

Sulphuretted hydrogen	1·21 cub. in.
Chlorine	970·92 grs.
Sulphuric acid	92·10
Lime	17·36
Magnesia	33·65
Magnesium	47·11
Sodium	553·06
Potassium	a trace
	1714·20
Amount of salts from evaporation	1718

Specific gravity, 1019·0.

4. "Water from the river Mooney, which empties itself in the north-east part of Corisco Bay, taken from about a mile inside the mouth, by Her Majesty's brig *Nautilus*, September 4th, 1839. Rain had fallen, but the rainy season cannot be considered to have set in."

This water did not smell of sulphuretted hydrogen, nor did it afford any trace of that gas upon analysis; the total amount of sediment in the bottle did not exceed 0·1 grain. It contained, per gallon,

Chlorine	1184·11 grs.
Sulphuric acid	109·80
Lime	14·17
Magnesia	44·78
Magnesium	28·54
Sodium	732·32
Potassium	a trace
	2113·72
Amount of salts from evaporation	2104

Specific gravity, 1022·5.

5. "Water from the river Gaboon, taken at four miles above Parrot and Konicky Island [eight miles up the river], by Her Majesty's brig *Nautilus*, September 10th, 1839. Rain

had fallen, but the rainy season was not considered to have set in. Latitude 0° 15′ north, longitude 9° 33′ east."

This water afforded no traces of sulphuretted hydrogen. The sediment in the bottle weighed 0·2 grain.

Chlorine	1130·75 grs.
Sulphuric acid	120·08
Lime	23·05
Magnesia	43·58
Magnesium	35·41
Sodium	683·00
Potassium	a trace
	2035·87
Amount of salts from evaporation	2060·0

Specific gravity, 1023.

6. " Water from Cape Lopez Bay, taken by Her Majesty's brig *Nautilus*, September 28th, 1839, when the Cape bore W. by N. about ten miles. The rainy season had commenced."

This water smelt very strongly of sulphuretted hydrogen. The sediment in the bottle weighed only 0·1 grain, and consisted of vegetable matter. It contained, per gallon,

Sulphuretted hydrogen	11·69 cub. in.
Chlorine	1467·37 grs.
Sulphuric acid	115·20
Lime	23·21
Magnesia	41·02
Magnesium	28·44
Sodium	921·60
Potassium	a trace
Iodine	a trace
	2596·84
Amount of salts from evaporation	2576·00

Specific gravity, 1026.

7. " River Congo,—water taken off Shark's Point, at the entrance of the river, by Her Majesty's sloop *Wolverine*, on the 11th of November, 1839, four days before the customary rains, but light rains having already taken place."

This water smelt very slightly of sulphuretted hydrogen. The sediment in the bottle weighed 0·4 grain, and consisted of vegetable matter. It contained, per gallon,

Sulphuretted hydrogen	0·67 cub. in.
Chlorine	106·11 grs.
Sulphuric acid	2·30
Sodium	70·00
	178·41

And small quantities of other bases.

Amount of salts from evaporation . 188

Specific gravity, 1002·0.

This is the only case in which the salts were discoloured by vegetable extractive matter.

8. "River Congo,—water taken about thirty-five miles up that river, by Her Majesty's sloop *Wolverine*, on the 11th of November, 1839, four days before the customary rains, but light rains having already taken place."

This water contained no sulphuretted hydrogen, and the sediment in the bottle was only 0·1 grain.

The amount of saline matter was only eight grains per gallon, and consisted of the chlorides of sodium, and magnesium, and sulphate of soda, chiefly.

Specific gravity 1000·3.

"Water from the river Bango, taken [at sea] at forty miles distance from the mouth, by Her Majesty's schooner *Fair Rosamond*, on the 26th of December, 1839, in latitude 8° 33′ south, and longitude 12° 41′ east." The water emitted a very strong smell of sulphuretted hydrogen. It was tolerably clear, but contained a little gelatinous matter which resembled spawn of fish. The sediment of the whole bottle, however, when dried, only weighed fifteen hundredths of a grain.

The results of the analysis, calculated for the imperial gallon, were as follows:—

Specific gravity, 1026·4.
Sulphuretted hydrogen, 4·35 cub. in.
Dry salts, 2736 grs.

Consisting of chlorine 1513, sulphuric acid 128, neutralized by bases, which have not yet been quantitatively determined, but consisting of sodium, magnesium, calcium, &c.

"Water taken by Her Majesty's schooner *Fair Rosamond*, off the Bango and Dande rivers, latitude 8° 29′ south, longitude 12° 33′ east, on the 29th of December, 1839."

Results of analysis,—specific gravity, 1026·7.

There was no odour of sulphuretted hydrogen in this water, neither was any detected by tests. The quantity of dry saline matter, per gallon, 2624 grains, consisting of chlorine 1430, sulphuric acid 125·4, neutralized by the same bases.

It is difficult to conceive how such a striking and important fact as the impregnation of the waters of the Ocean, upon such a long line of coast, with this deleterious gas, should so long have escaped observation.

Upon searching for evidence of a similar phænomenon having been observed before, I have found in the *Phil. Trans.* for 1819, a memoir by the late Dr. Marcet, "On the Specific Gravity and Temperature of Sea Waters in different parts of the Ocean, and in particular Seas, with some account of their saline contents." Out of sixteen specimens which he examined he found one which was brought by Capt. Basil Hall from the Yellow Sea in the Chinese Ocean, which, from the account which he has given, must probably have been as highly charged with sulphuretted hydrogen as those from the coast of Africa; and he observes, "there is something in the development of sulphur in sea water which is by no means well understood."

He also noticed that a specimen obtained by Mr. Schmidtmeyer, going to South America, from lat. 10° 50′ N., long. 24° 26′ W., had an hepatic smell, and had blackened the bottle in which it was contained.

Since the report, which I had the honour to make to the Admiralty, Sir William Burnett has been kind enough to furnish me with the analysis, by Mr. Garden, of some bottles of water, collected by Dr. Mc William, of Her Majesty's ship *Scout*, in March 1839, in the river Bonny, of which the following are the particulars:—

1. Water from the river Bonny, taken half a mile inside the mouth, on the 12th of March, 1839, just before the commencement of the rainy season.

Each imperial pint of this water contained 245 grains of saline matter, consisting of the following ingredients:—

Sulphate of Magnesia
Do. of Lime
Muriate of Magnesia
Do. of Soda

Sulphuretted hydrogen, ·680 cub. in. (5·44 cub. in. per gall.)

2. Water from the mouth of the river Lagos, lat. 6° 20′ N., long. 3° 30′ E. nearly.

Each pint yielded 240 grains of saline matter, consisting of the same ingredients as those above mentioned; also Sulphuretted hydrogen, 1·844 cub. in. (14·752 cub. in. per gall.)

3. Water from the river Bonny, taken off Ju-ju Point, on the 12th of March, 1839, about one mile and a half within the mouth.

One pint yielded 200 grains of saline substances, as above, and Sulphuretted hydrogen, ·505 cub. in. (4·04 cub. in. per gall.)

4. Water from off Grand Bonny, about three miles inside the mouth of the river.

One pint yielded 208 grains of the same salts as above, and Sulphuretted hydrogen, 3·500 cub. in. (28·0 cub. in. per gall.)

These results appear to have attracted no particular attention at the time when they were obtained. The old hypothesis of the corruption of the specimens probably sufficed for their explanation upon this as upon other occasions.

Thus there can be no doubt of the important fact of the impregnation of the waters, upon the western coast of Africa, with sulphuretted hydrogen, to an amount, in some places, exceeding that of some of the most celebrated sulphur springs in the world; and of the injurious effect of such impregnation upon the copper sheathing of ships, you will be convinced by the experiments upon the table.

Were any further evidence wanting, it would be found in the state of the copper of the *Bonetta*, which lately returned from the coast of Africa, and three sheets of which were sent to me from the Admiralty for examination.

Nos. 1 and 2 were pretty uniformly covered on the outside with a green crust; and on the inside, as evenly, with a black crust of equal thickness. They were very thin in parts, and here and there eaten into holes.

No. 3 was in a much worse state, very thin, and eaten into large holes. In most parts it was easily broken by the fingers; one of the holes, of an irregular shape, measured eighteen inches in length by four inches and a half in width. This sheet was covered with green crust chiefly, on both sides; but there were evident traces of the black crust on the inner side.

Upon analysis the black crust was found to consist of sulphuret of copper, and the green of oxy-chloride of copper.

There can be no doubt that the injury to the copper arose, primarily, from the sulphuretted hydrogen. The gas appears to have penetrated to the inner side of the copper, where in Nos. 1 and 2 it has been protected from the further action of the sea water; by which, on the outside, the sulphuret was converted into chloride of copper. This conversion appears to have taken place on both sides in No. 3, from the sea water having penetrated to the under side in consequence of its greater corrosion.

That the establishment of this fact is of some importance in a mercantile point of view, I think I shall be able to convince you by two anecdotes which I will now narrate.

Not many years ago a new copper company set up a smelting establishment and brought their copper to market: some merchants purchased sheathing of them, coppered their ship,

and sent her to the coast of Africa. Not many months after she returned to this country with the copper in the same state as that of the *Bonetta.* The merchants said—the copper-smelters were inexperienced hands—they did not know their business—the sheathing was improperly made; and they brought an action against the Company, who defended it.

Upon the trial some of the most eminent scientific men of the day gave evidence that there was nothing in sea water which could produce such rapid decay of the copper, and the jury, in consequence, brought in a verdict for the plaintiffs.

Now contrast this with what has happened to me within the last two months. An eminent copper manufacturer of South Wales, who had heard nothing of the investigations in which I had been engaged, came to me with two samples of copper which he wished me to analyze. The one was of new metal, and the other part of the sheathing of a ship which had just returned from Africa, after a voyage of a few months, the copper being in a state of utter decay. He stated that the merchants to whom the vessel belonged had brought an action against him on the plea that the copper was imperfect, and he wished for my evidence upon the subject, as he well knew that the copper was perfectly good. Instead of entering upon the analysis I gave him a copy of my report upon the waters of the western coast of Africa, which he sent to the merchants, and nothing further has been heard of the action.

But it may perhaps be said that little good will arise from pointing out the evil, unless we are prepared to propose some remedy for it; not that I agree to this, for the existence of the sulphuretted hydrogen is so readily tested, even by the roughest hands, that nothing can be easier than to ascertain and avoid the localities in which it prevails; motives for which course I shall presently mention, of greater weight even than the preservation of the copper.

But I think that the remedy is certainly within our command. The principle of protection proposed by Sir H. Davy is quite applicable to it, with some additional precautions suggested by this newly-discovered destructive agent, which had escaped his notice.

It is well known that his experiments were conducted chiefly with zinc and iron, as the active elements of protection; and he was led ultimately to the adoption of cast iron, "as the substance which is cheapest, most easily procured, and likewise most fitted for the protection of the copper*."

But this is not the case with regard to sulphuretted hydro-

* Phil. Trans., June 1824, p. 243; [or Phil. Mag. First Series, vol. lxv., p. 204.]

gen; for as you will see by reference to the experiments upon the table, copper is much more acted upon by this substance than iron, the latter being protected by the former; and the fact is, that a piece of iron attached to copper increases the corrosion of the latter.

Zinc, on the contrary, protects the copper not only from the action of the chlorides in sea water, but also from the sulphuretted hydrogen. This I have ascertained by the experiments before you, and you will find that the results are in perfect accordance with the electric order of these two metals in solutions of the hydrosulphurets, as given by Dr. Faraday in his last beautiful number of Experimental Researches in Electricity.

In the table which he gives, iron stands far above copper in electro-negative order, and zinc below it; lead is also above zinc; while in the usual acid solutions both zinc and iron stand below that metal*.

Now I have long been of opinion that the experiment of voltaic protection in the Navy was much too lightly abandoned upon the first appearance of an unforeseen difficulty, and that under circumstances otherwise the most encouraging.

This abandonment, you are aware, arose from what might be called over-protection, by which the attachment of weeds and zoophytes to the ships' bottoms was found to be encouraged. Earthy deposits were formed, and to these the weeds and shell-fish attached themselves.

The remedy for this appears to me to be obvious: instead of keeping the protectors always in contact with the copper, let them be insulated, and let them only be brought into metallic contact when occasion may require. This might readily be done by means of a bolt or bar, forming in one position a continuous conductor between the two metals, and in another breaking the connexion: this might always be at the command of the proper officer of the ship. Nothing could then be easier than to throw off the protection when the ship is in harbour, or in situations particularly liable to deposits; or to restore it upon going to sea, or arriving in latitudes where sulphuretted hydrogen might be found to exist.

But the protectors should invariably be of zinc, which would preserve the copper not only from the effects of sea water generally, but from the more destructive agency of sulphuretted hydrogen, which I shall presently give you my reasons for concluding not only prevails upon the western coast of Africa, but in other situations where it has never yet been suspected. Indeed I incline to believe that it would

* Phil. Trans., 1840, p. 113.

only be found necessary to use protection in sulphuretted waters, and that the action of the chlorides alone might not be more than sufficient to preserve the copper from deposits.

Another motive for this change may be found in an observation of Sir H. Davy, viz. that a "common cause of the adhesion of weeds or shell-fish is the oxide of iron formed and deposited round the protector. In the only experiment in which zinc has been employed for this purpose in actual service, the ship returned, after two voyages to the West Indies and one to Quebec, perfectly clean. The rudder, which was not protected, was corroded in the usual way*."

But it is impossible not to speculate upon the origin of the deleterious gas which has thus been found to contaminate the sea upon the western coast of Africa, in such enormous quantities through an extent of more than sixteen degrees of latitude, and reaching in places forty miles seawards, making altogether an area of 40,000 square miles in extent.

Volcanic action seems naturally to suggest itself, but is negatived by the absence of any other indications of such action along this line of coast; and I think that I shall be able to convince you, by the evidence of experiment, that the real cause may be found in the mutual reaction of the immense quantities of vegetable matters, which must be brought down by the intertropical rivers, and the sulphates of the sea water.

The idea was suggested to me by a memoir of my friend Dr. Malcolmson, in the Geological Transactions, who speculates upon the origin of sulphuretted hydrogen in the saline lakes of different parts of the world, being "the decomposition of the sulphates in the water by the carbonaceous matter of vegetables." I tested this hypothesis by experiments in the following way:—

On the 2nd of November, 1840, I placed a quantity of newly-fallen leaves in three glass jars capable of holding about a gallon and a half of water.

No. 1. Upon the first I poured about a gallon of New River water.

No. 2. Upon the second I poured about the same quantity of the same water, in which three ounces of common salt had been dissolved.

No. 3. Upon the third, the same quantity of water, in which three ounces of crystallized sulphate of soda had been dissolved.

The three jars were then placed in a warm chamber, the

* Phil. Trans. 1826, p. 420; [or Phil. Mag. Second Series, vol. i., p. 198.]

temperature of which varied from about 70° to 110°, and the water was filled up from time to time, as it evaporated, and the mixture well stirred.

Upon examining them on the 5th of February, 1841 (three months), the following was found to be the state of the jars:—

No. 1 had a very disagreeable odour, but produced no change whatever upon paper soaked in acetate of lead.

No. 2 was perfectly sweet, and possessed, indeed, a rather agreeable odour. It produced no effect, of course, upon the test paper.

No. 3 had a most insupportable sickening odour, much worse than that of pure sulphuretted hydrogen, and instantly blackened paper soaked in acetate of lead, throwing down sulphuret of lead with a metallic lustre.

You will have an opportunity of observing, by the specimen upon the table, that the evolution of the gas is at this moment proceeding with increased energy.

Now, the analysis of sea waters generally, and these analyses in particular, show that a large proportion of sulphates is always present in them, and there is no doubt that extensive mud-banks must be formed at the mouths of the African rivers, within the tropics, consisting chiefly of vegetable detritus, in the exact state which is most favourable to this action.

Since my report to the Admiralty upon this subject, I have seen a paper in the *Annales de Chimie* for July 1840, by Dr. Amédée Fontan, upon the Mineral Waters of Germany, Belgium, Switzerland, and Savoy, in which he suggests that the presence of sulphuretted hydrogen in those waters may be owing to the decomposition of the sulphates which they contain by vegetable matters, remarking that many of them which contain little of that gas at their sources, acquire more of it by their flow through the soil. There can be little doubt of the correctness of this opinion.

A curious fact has also been brought to my recollection by my friend Mr. Fownes, with regard to a spontaneous change which a solution of litmus undergoes when excluded from the air. It becomes of a brown colour, but still it is not spoiled, for the colour is restored by exposure to air.

M. Vogel (Ed. Journ. 31, 157), who inquired into this curious fact, found that the solution always contains sulphate of potassa, which becomes gradually decomposed with the generation of sulphuretted hydrogen, to the deoxidating power of which the effect is owing. A few drops of sulphuretted hydrogen solution produces the same effect in a few days; the solution becomes brown, but speedily recovers its colour upon

contact with air. This case is the more interesting, inasmuch as the gas never exists in sufficient proportion to be discoverable by the usual tests; nevertheless it acts in these minute quantities with great energy.

But now a much more important and interesting question than that of the preservation of the copper sheathing of ships forces itself upon our attention, and that is, whether the existence of this deleterious gas in the atmosphere, which must necessarily accompany its solution in the waters, may not be connected with that awful miasma which has hitherto proved so fatal to the explorers and settlers of the deadly shores of Africa; and whether, if so, science may not suggest something to palliate an evil which is so dreadfully opposed to the progress of civilization in those parts.

When this matter was first brought under my consideration, I was surprised that the nauseous smell which must necessarily be evolved from water impregnated with this gas, at so high a temperature as that of the equinoctial regions, had not been noticed. I have in consequence turned to some of the accounts of the late travels in Africa, to seek for evidence upon the subject; and in the narrative of an expedition into the interior of Africa, by the river Niger, by Macgregor Laird and R. A. B. Oldfield, I found the following important observations:—

"The principal predisposing causes of the awful mortality, were in my opinion the sudden change from the open sea to a narrow and winding river, the want of the sea breeze, and the prevalence of the deadly miasma, to which we were nightly exposed from the surrounding swamps. *The horrid sickening stench* of this miasma must be experienced to be conceived: no description of it can convey to the mind the wretched sensation that is felt for some time before and after daybreak. In those accursed swamps, one is oppressed not only bodily but mentally with an indescribable feeling of heaviness, languor, nausea, and disgust, which requires a considerable effort to shake off."

Now, these observations were made in the very locality from which some of the first waters which I examined were taken, and nothing more is wanting to identify the cause of the rapid decay of the ship's copper with that of the mortality of the climate.

It has been experimentally found, that so small a mixture as a fifteen hundredth part of sulphuretted hydrogen in the atmosphere, acts as a direct poison upon small animals, and the sensations of languor and nausea, described by Mr. Laird, are exactly those which have been experienced by persons

who have been exposed to the deleterious influence in small quantities.

The symptoms, in cases where this gas is breathed in a state of concentration, well known to medical men, are sudden weakness, and all the signs of asphyxia: the individual becomes suddenly weak and insensible; falls down, and almost immediately expires. When the exposure has been too slight to cause serious mischief, the individual is affected with sickness, colic, imperfectly defined pains in the chest, and lethargy.

Now, can it be deemed at all improbable, that an agent which is capable of acting with this severity as a direct poison, when mixed in no very high proportion with the atmosphere, should in still less quantities greatly aggravate symptoms of morbific action, which may possibly have their origin in other causes?

In the very expedition, from the account of which I have already quoted an extract, a circumstance occurred which is almost an experimental confirmation of these views. The first sickness and death in that expedition began at Cape Coast Castle; three died before entering the river, and the great mortality took place before they reached Damuggoo at the extreme upper end of the Delta, where they only arrived after a voyage of thirty-six days, from the 11th of October to the 16th of November, or twenty-seven from their entrance of the river Nún.

Now it is worthy of remark, that just before entering the river, in "breaking out" the hold to lighten the vessel, it was discovered that the cause of a "disagreeable vapour, from which they had long suffered, was, that the bags containing the cocoa had rotted, and the cocoa had fallen into the salt bilge-water and there become putrid." Here, then, were the very ingredients for generating sulphuretted hydrogen to a great extent; the lamentable consequence has been before alluded to, namely, three deaths before reaching the river. There can indeed be no doubt that the disagreeable effluvium of bilge-water, which has been carelessly left undisturbed for a long time, is owing to similar decomposition.

It is doubtless the same circumstance which renders Mangrove swamp so notoriously pestilential in all parts of the torrid zone. The tree only thrives in salt water, and its decayed foliage is admirably adapted to act upon the sulphates; and it accounts for the observation, that malarious fevers diminish as we recede from the coast, although swamp and rank vegetation may still prevail.

The close investigation which I have since given to the

subject, more and more convince me that the worst cases of *malaria* are generally connected with the presence of sulphuretted hydrogen.

There is a paper in the twenty-ninth volume of the *Annales de Chimie*, p. 225, by Signor Gaetano Giorgini, which offers the strongest possible confirmation of my opinion.

"The observation of Signor Giorgini has been drawn to the state of the atmosphere in the neighbourhood of certain marshes on the borders of the Mediterranean; and by reference to historical data, and various documents, he has proved the great importance which attaches to the circumstance of their being, at times, in communication with the sea, so as to have a mixture formed between their waters and that of the sea. Both ancient and modern authors have announced the fatal effects produced in the neighbourhood of marshes by such mixture, and a local belief of the same thing is very common and strong.

"On the south of the Ligurian Apennines is a marshy shore, bounded on the west for twelve miles by the Mediterranean, on the south by the river Serchio, and on the north by the river Frigido, a torrent commencing at the foot of the Apennines, in the state of Massa di Carrara, running three or four miles over the land, and then falling into the sea. The plain is from two to four miles wide, and is traversed by a few short torrents or streams; among these are the rivers Camajore and Pietra Santa, which divide the plain into three separate basins. The rain and spring waters which flow into the three basins mentioned, are slowly discharged into the sea by natural or artificial canals, penetrating the sand-bank, which exist on the sea-side.

"The level of these stagnant waters is between that of high and low water in the neighbouring sea; there being but little difference between these two points in this part of the Mediterranean. In this state of things, formerly, when the waters of the sea arose from any circumstance (unless the waters of the marshes were very high), they used to return up the ditches, fill the basins, and inundate the country to the foot of the mountains; and with a north-west wind, the waves used to penetrate with force to the interior. The mixture of fresh and salt water thus formed, and which, in summer, was rarely changed, became corrupt, and spread infection over the neighbourhood of the most destructive kind.

"In this way the effects of the malaria were reproduced annually in the neighbouring country, with all their peculiar horrors: the population, though small, presented feeble infants

and diseased men, old age being unknown there. All attempts to avoid the scourge, by living on the hills, or in the interior, and frequenting the plain when the business of cultivation essentially required it, were vain; they fell victims to the extensive influence, and such being the effects upon the inhabitants of the country, much more rapidly did a stranger suffer from the deleterious atmosphere; one single night, in the months of August and September, causing inevitable death to the incautious traveller who should stay so long in this infested country.

"Such was the state of things until 1741. Previous to that time, Gemignano Rondelli, Eustachio Manfredi, and Bernardino Zendroni had successively insisted upon the necessity of excluding the sea from these marshes; and, in 1740–41, a sluice, with folding doors, competent to give emission to the waters of the marsh, but prevent the sea from entering, was constructed at the mouth of the Burlamacca. The most complete and unexpected success immediately followed upon, and has continued with, this work. The year after its completion there were no appearances of the terrible maladies which previously appeared every year. The inhabitants soon recovered health, and the land being very fertile, the population rapidly increased, and is increasing at this moment. Viareggio has become a considerable town, and so completely has all suspicion of its insalubrity disappeared, that the first families of the city of Lucca have for years built their summer seats there. Notwithstanding the success of the precautions taken at this part of the coast, the neighbouring parts were long left a prey to the destroying influence of the mixed marsh-waters; and the inhabitants around the basins of Motrone and Perotto were not considered until the year 1804. In the years 1809, 1810, 1811, similar means were taken, with the best effects to the inhabitants of Montignosso and the vicinity; and, in 1812, a sluice was constructed on the Cinquale, which perfected the arrangements in this part, and made a large portion of country equally healthy with Viareggio. To complete the arrangement, it was now only required to guard the ditches of Motrone and Tonfalo with sluices; the former was finished in 1819, and the latter in 1821. Since that time the diseases of malaria have ceased so entirely at all points, that no other dangers are now incurred regarding the insalubrity of the atmosphere than such as may arise from neglect of these sluices, which the inhabitants of the country should regard as their palladium."

I should weary you were I to multiply examples of the

existence of the worst forms of malaria in places which we now know combine all the necessary circumstances for the generation of sulphuretted hydrogen.

In Mr. Darwin's interesting Journal of the Voyage of the Adventure and Beagle, he speaks of several such places, especially in Peru, in connexion with the well-known effects of miasma, but not with any suspicion of the real origin of the evil. He mentions repeatedly the efflorescence of the sulphates of magnesia and soda upon the soil, and that the mud of the saline lakes is "black, and had a fœtid odour." The inhabitants suffer in such situations from the worst attacks of ague. He remarks, p. 447, "The attacks of illness which arise from miasma never fail to appear most mysterious. The miasma is not always produced by a luxuriant vegetation with an ardent climate; for many parts of Brazil, even where there are marshes and a rank vegetation, are much more healthy than this sterile coast of Peru."

"So difficult is it to judge from the aspect of a country whether or not it is healthy, that if a person had been told to choose within the tropics a situation appearing favourable for health, very probably he would have named this coast."

Its peculiarity consists in the saline efflorescence upon its soil, of which the sulphates of magnesia and soda constitute a very great proportion.

May not the jungle fever in India depend for much of its malignity upon the same cause? The soil in many parts abounds not only with the nitrates of potassa and soda, but the sulphates of soda and magnesia; these become washed down by the periodical rains, and mingling with the decaying leaves, the mutual reaction takes place.

Is it not worthy again of the most exact inquiry, whether the fevers which periodically afflict the cities of New York and Charlestown, in America, may not be connected with the mixture of animal and vegetable substances with the sea water in their lower districts, where they usually originate; and whether an attentive examination will not prove, that the same impregnation of sulphuretted hydrogen, which we have established upon the African coast, exists at the mouth of the vast rivers of the American continent? Indeed I have been informed by an officer high in the naval service, that during the war instances of the rapid decay of ships' copper, similar to that upon the African, were noticed upon the West-Indian station.

To bring the matter nearer home. Is it impossible that the minor insalubrity of parts of our own coast, such as that of Essex, may have some reference to the same causes acting

in minor degrees. The mud of the river Thames may surely produce the same decompositions of the sulphates in the sea water as that which we have traced in other places. I find that there is a prevalent idea, which deserves investigation, that the ships in the Medway are liable to greater waste of their copper than those at Woolwich, or other places on the Thames.

Within the past week I have seen in the number of Liebig's *Annalen* for January last, that Dr. Clemm examined some water taken up in December 1839, in the open sea off Barmouth, North Wales; also from Aberystwyth and Tenby in South Wales; and he observes, "In some of the bottles from the English coast the large proportion of sulphuretted hydrogen was very striking: both in smell and taste it was converted into a strong sulphurous water." He seems to think that the accidental presence of some of the sealing-wax with which the bottles were closed, may have had something to do with the generation of the gas.

By the kindness of Sir Isambard Brunel, I have been favoured with the particulars of a very remarkable phænomenon of this kind, which attended the progress of the Thames Tunnel, and which proved of great annoyance, and was the cause of great suffering to some of the workmen. This was the evolution of great quantities of carburetted hydrogen mixed with sulphuretted hydrogen gases. In 1837 the smell was extremely fœtid. "It frequently rushed out from between the small boards of the shield, used to scour the ground with great force, bursting into a flame, which sometimes extended over some feet, and was generally accompanied by a loud roaring or hissing noise."

In 1838, the sulphuretted hydrogen was unaccompanied by the great quantity of carburetted hydrogen with which it had been previously mixed, but its effects upon the men were very severe, producing sickness, giddiness and fainting, pains in the head, and in one case delirium.

The source of this enormous production of gas may probably have been the deposits of animal and vegetable matters in the mud of the river, acting however by the sulphates, which are contained in the former, in a manner perfectly similar to that which we have traced to the sulphates in the sea water.

And here again it may be asked, as with regard to the injurious effects of sulphuretted hydrogen upon the copper of ships, can science indicate a remedy, as well as point to the cause of the disease? And again I would reply, that by furnishing an easy method of detecting the evil, she furnishes you with timely warning to fly from the infected regions. No vessel should be allowed to cast anchor or linger in sulphu-

retted waters. But if paramount duty should oppose itself to such a course, we have a certain remedy to propose. You have seen how instantly chlorine destroys the gas. Chlorine and sulphuretted hydrogen cannot coexist together. Plentiful fumigations of chlorine would therefore infallibly prevent the deleterious effects; and the antidote is at once cheap, and incapable, under proper management, of producing any injurious effects to counterbalance its advantages.

The Lords of the Admiralty have received these suggestions with indulgence, and have given instructions to their cruisers upon the African coast to test the waters at regular intervals. They have also abundantly supplied the African expedition with the means of chlorine fumigation; and I have the gratification of knowing that the views which I have now had the honour of submitting to you, have tended to give confidence not only to the gallant band who have devoted themselves to one of the most disinterested enterprises which ever emanated from pure Christian charity, but to the numerous friends who wait the result with anxiety.

II. *Abstract of recent Researches on the quantity of Heat evolved in Chemical Combination, particularly those of* MM. Dulong *and* Hess.

FARADAY having established the law of definite electrolytic action, and thus connected together the two principles under which scientific chemistry at the present day is organized, the abstract and the numerical, the electro-chemical theory and the law of equivalents, it became an interesting point to examine whether affinity possessed the same definiteness of character in relation to other physical agents, and especially to heat, the influence of which on affinity is even more remarkable than that of electricity, and with whose operations indeed chemistry itself, until a comparatively recent date, was identified.

Investigations tending to this end had been long since entered on. The measurement of the quantity of heat evolved in combustion was, independent of all relations to scientific theory, a problem of such technical importance, that it has occupied from time to time all those philosophers who devoted themselves to that branch of science. Their results, frequently discordant, but still often useful, have been considered, especially those of Despretz, as indicating that the heat evolved was proportional to the quantity of oxygen which entered into combination.

For some time before his death, M. Dulong had been occu-

pied with researches of this kind, and the attention of the Academy of Sciences having been directed thereto by M. Hess, who had had conversations with Dulong on the subject, the manuscripts which he left behind were examined by Arago, and the following results obtained. Dulong had made an extensive series of experiments on the quantity of heat evolved in combustion, but did not write out any full description of his apparatus or methods; M. Cabart, however, who had been working in Dulong's laboratory, was able to describe the form of calorimeter which had been used. A rectangular box in red copper, of 25 centimetres in depth, 7·5 in breadth, and 10 in length, is the space in which the combustion is carried on; the oxygen or other gas supporting the combustion is introduced by a tube, and the products of combustion carried off by another. For the precise arrangement of these tubes, which are complicated in their form, for the purpose of receiving the thermometers by which the temperatures are read off, we must refer to the original. This box is immersed in a vessel holding eleven quarts of water. In this box gases were burned by means of jets, of various sizes, according to the combustibility of the gases. The liquids were burned by means of a few cotton threads in a glass tube closed at one end: it is not known how they were set on fire. The solid bodies (except iron, which was used in coils of fine wire) were in the state of powder, and were set on fire by a bit of amadou.

The following tables give the most important results obtained by Dulong; the unity of calorific effect employed being the quantity of heat necessary to raise the temperature of a gramme of water one centigrade degree at ordinary temperatures.

Substances and quantities employed.	Quantity of heat evolved in combustion. Max.	Min.	Mean.
One litre of hydrogen, five experiments	3120	3075	3102
One litre of pond gas, four experiments	9948	9317	9587
One litre of olefiant gas, five experiments	15576	15264	15338
One litre of carbonic oxide, three experiments	3202	3069	3130
Carbon, equivalent to one litre in gaseous state, four experiments	8040	7540	7858
Alcohol, one litre in vapour, two experiments.	14441	14310	14375

Substances and quantities employed.	Quantity of heat evolved in combustion. Max.	Min.	Mean.
Oil of turpentine, one litre in vapour, one experiment	70607		
Æther, one litre in vapour, two experiments	33968	32738	33254
One litre of cyanogen, three experiments	12602	12080	12269
One gramme of sulphur, three experiments	2719	2452	2602

The following simple bodies evolved by combustion in oxygen for every litre of oxygen used, the following proportional quantities of heat:—

Bodies.	No. of Exps.	Max.	Min.	Mean.
Iron	2	6281	6152	6216
Tin	3	6790	6325	6508
Protoxide of tin ...	3	6611	6262	6405
Copper	3	3742	3503	3578
Suboxide of copper.	1	3130		
Antimony	5	5875	5348	5552
Zinc	3	7753	7378	7577
Cobalt	1	5721		
Nickel	1	5333		

These results want probably the great accuracy which Dulong would have given to them had he been longer spared to science; but they probably fluctuate round the true numbers. We cannot judge of what general conclusions he would have deduced from them; but one important consequence appears to follow naturally from the numbers now given, since it appears that the same quantity of oxygen does not evolve the same quantity of heat in combining with various bodies. Thus if we deduce from the first table the quantity of heat evolved by each substance in perfect combustion with a litre of oxygen, we obtain the following numbers:—

Hydrogen	6204
Carbon	7858
Pond gas	4793
Olefiant gas	5113
Alcohol	4792
Carbonic oxide	6260
Oil of turpentine	5043
Æther	5542
Cyanogen	6135
Sulphur	3744

Amongst the metals similar deviations occur.

Hess states, in his letter to Arago, that he had taken notes immediately after a conversation with Dulong, in which the latter said he had arrived at the following general laws:—

1st. That the quantities of heat evolved are (nearly) the same for the same substance, no matter at what temperature it burns.

2nd. Equal volumes of all gases, in combining with oxygen, evolve the same quantity of heat.

3rd. The same quantity of heat is disengaged with the same quantity of oxygen, whether it forms a compound R + O, or R + 2 O.

4th. The quantity of heat disengaged by different substances is very different.

These conclusions, however, are by no means completely borne out by the experimental results, so far as they have been recovered, except the third, which is certainly supported by the results with tin, protoxide of tin, carbon and carbonic oxide.

In commenting on these numbers, Berzelius has pointed out a remarkable consequence deducible from them; that is, that the heat evolved in the combustion of a compound gas is the same as that evolved in the combustion of its constituents. Thus,

One volume of pond gas consists of two volumes of hydrogen and half a volume of carbon (vapour).

	Mean.	
Two volumes of hydrogen produce heat	= 6204	
Half a volume of carbon (vapour)	= 3929	
		10133
The highest experimental number is . . .	= 9948	

One volume of olefiant gas consists of two volumes of hydrogen and one of carbon (vapour).

	Mean.	
Two volumes of hydrogen evolve heat . .	= 6204	
One volume of carbon (vapour).	= 7858	
		14062
The mean experimental result is	= 15338	

One volume of oil of turpentine vapour consists of eight volumes of hydrogen and five of carbon (vapour).

	Mean.	
Eight volumes of hydrogen evolve heat .	= 24816	
Five volumes of carbon (vapour).	= 39290	
		64106
The experimental number is	= 70607	

The differences here found are not probably greater than the limits of experimental error; their direction also is not constant.

The laws of phænomena are very frequently more easily

examined and established when attention is directed to those cases in which they act with but moderate activity; the results, being less sudden and turbulent, become more easily subjected to accurate measurement and observation. It has been remarkably so with electricity; and this it is which gives peculiar value to the thermo-chemical researches recently entered upon by Hess and Andrews. In Poggendorff's *Annalen* in 1839, Hess communicated some results on the admixture of oil of vitriol and water, and announced that *when two substances unite in many proportions, the quantities of heat evolved in these acts of combination are multiple proportions of each other.* To the further development of this law he has since devoted himself, and obtained a great number of important results, which we will now proceed briefly to describe.

The instances which first conducted M. Hess to the detection of this law, were found in the quantity of heat evolved by the combination of oil of vitriol with water. The method first pursued was as follows:—The strength of the dilute sulphuric acid being determined, which did not evolve heat by any further addition of water, and quantities of oil of vitriol being diluted with water so as to bring them to various atomic constitutions, these were added to the quantities of water necessary to bring them to the standard first described, and the rise of temperature consequent on the mixture accurately determined. The two liquids (the water and atomically diluted oil of vitriol) had accurately the same temperature before mixture, and the weights were accurately known.

In order to compare the results given by a series of experiments, some arbitrary unit should be taken, and for this was selected one part of sulphuric acid considered as anhydrous; for instance, if the experiment was made with $S\,O_3 + 3\,H\,O$. Since the quantity of this last was known, the quantity of dry sulphuric acid therein contained was also known, and let it be marked 4. Then the heat evolved in the experiment, divided by 4, gives the quantity of water which should have been raised one degree of temperature by one part of anhydrous acid. In this way the results which follow, and which were announced by M. Hess in 1839, were obtained.

Acids.	Units of Heat.	Multiples.	Corrected Units of Heat.
$S\,O_3 + 6\,H\,O$	43·8	2	39·4
$S\,O_3 + 4\,H\,O$	67·2	3	60·4
$S\,O_3 + 3\,H\,O$	93·5	4	84·1
$S\,O_3 + 2\,H\,O$	132·6	6	119·34
$S\,O_3 + \;\;H\,O$	227·5	10	204·75

The fourth column contains the number of units of heat

evolved after the results had been corrected for the specific heat of each acid used.

For this simple method of mixtures M. Hess subsequently substituted the use of a calorimeter, devised by him for the purpose. This instrument consists, 1st, of a box for holding water; inside of this is, 2nd, a cylinder, fitted, as well as its cover, with a broad edge; these edges are ground and fitted tight to each other by eight screws. The outer surface of the cylinder is fitted with projecting copper wings, which serve to agitate the water in the external box, leaving free, however, a space, which admits of the entrance of the thermometer. The cylinder is supported horizontally on its axis, and is fitted on one side outside of the box with a winch handle, by which it may be made to revolve rapidly.

The mode of using this instrument is as follows: the cylinder being taken out is placed steadily in a wooden support, and the cover screwed off. Whichever of the two liquids to be mixed occupies least volume, is to be then introduced into a glass or copper vessel and laid in the cylinder; the other liquid is to be then poured in, the cover fastened, and the cylinder laid in its place, surrounded by the water of the cylinder. On then turning the cylinder rapidly round by the handle, the inside vessel overturns, the liquids mix, and the heat evolved is imparted to the water, outside of which the temperature is accurately determined by a Collardeau thermometer. The change of method when one substance is solid can easily be appreciated.

By means of experiments conducted with this apparatus, it was found, after applying all necessary corrections, that

				Units of Heat.
$HO + SO_3$	combining with	HO	evolved	77·17
$HO + SO_3$	,,	$2HO$	,,	116·7
$2HO + SO_3$	,,	HO	,,	38·56
SO_3	,,	x, HO	,,	510·1

From these results, combined with those already given, the following final results may be deduced for the quantities of heat evolved in the formation of diluted sulphuric acid of definite constitution:—

Acid.		Heat evolved.	Multiples.
$SO_3 + HO$		310·4	8
$HO.SO_3 + HO$		77·86	2
$(2HO + SO_3) + HO$		38·9	1
$(3HO + SO_3) + 3HO$		38·9	1
$(6HO + SO_3) + xHO$		38·9	1
Total for $SO_3 + xHO$	=	504·96	13

And from hence the heat produced by mixing sulphuric acid of any strength, with an excess of water, may be computed.

Acid.	Theory.	Experiment.
SO_3	504·96	510·1
$SO_3 + HO$	194·5	204·75
$SO_3 + 2HO$	116·7	119·34
$SO_3 + 3HO$	77·8	84·1
$SO_3 + 6HO$	38·9	39·4

In extending these researches to the combinations of other bodies than water with sulphuric acid, M. Hess arrived at the remarkable result, that *when a combination takes place the quantity of heat evolved is constant, whether the combination be direct or indirect; whether it takes place at once or by successive stages.* This principle is supported upon evidence derived from an extensive series of experiments, to the description of which we shall now pass.

[To be continued.]

III. *Further Experiments on the Electricity of Effluent Steam.* *By* W. G. Armstrong, *Esq.**

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In pursuing my experiments on this subject, I observed that the tendency of the steam cloud to evolve *negative*, instead of *positive* electricity, gradually increased as I continued to use the apparatus, until at length positive electricity rarely appeared in the jet, even when circumstances were most favourable to its development.

Conceiving that this gradual predominance of negative electricity in the steam might possibly be owing to the progressive oxidation of the metal in contact with the water, I examined the interior of the boiler; but no change appeared to have taken place in the surface of the metal, which was in the rough state in which castings come from the mould. I then washed out the boiler with water, but upon using it again the electricity of the steam remained negative as before. After this the boiler was washed out with a solution of potash, by which means I was agreeably surprised to find the positive electricity of the jet was completely restored. This remarkable result led me to try the effect of dissolving a little potash in the water from which the steam was generated, and by so doing the quantity of electricity was amazingly increased; so much so, that I was enabled to obtain from the apparatus upwards of thirty sparks, half an inch long, in a minute.

* See our preceding volume, p. 328.

Having thus discovered an instance in which the electricity was so strongly affected by the contents of the water, I proceeded to try the effects of other ingredients, and the following are the results I obtained:—

Soda had much the same effect as potash, rendering the electricity of the steam positive, but not occasioning quite so strong a development. A minute quantity of nitric acid produced the contrary effect, causing the steam to be *negatively* electrified. Muriatic acid had no perceptible influence; neither had sulphuric acid, not even when iron filings were contained in the boiler. Lime tended to produce positive electricity, but not powerfully. Common salt appeared to have no effect. Nitrate of copper rendered the steam negative, in nearly the same degree as nitric acid.

Potash, soda or lime used in excess, always occasioned what engineers call *priming*; which is the ejection of water from the boiler in conjunction with the steam. Whenever this took place the electricity either vanished entirely, or became exceedingly feeble. It is therefore unintelligible to me how it happens that Dr. Schafhaeutl obtains most electricity when the steam is combined with water.

From the powerful effects I obtained, when water containing a little potash was used, I have little doubt that a properly-constructed electro-steam apparatus would answer all the purposes of an electrifying machine. My own apparatus, though small and imperfect, is no mean substitute for an electrifying machine; and if sharp edges, points, and asperities had been avoided, its efficacy would undoubtedly have been much greater than it is. A gallon of water is the utmost the boiler will hold, so as to leave sufficient room for steam; but a boiler capable of holding *ten* gallons would not be at all inconveniently large; and, with a duly proportioned extent of heating surface, would afford *ten* times as much steam, and consequently *ten* times as much electricity, as mine. There is no occasion to adhere to the form I have adopted for my boiler, which form is only eligible on account of its strength. Experience has shown that there is not much to be gained by increasing the pressure of the steam beyond 60 or 70 pounds on the square inch; and a boiler adapted to resist this pressure would be found sufficiently strong. The coil of copper pipe appended to my apparatus might be dispensed with, for I have always obtained the strongest effects by discharging the steam through a glass tube 6 or 8 inches long. It is important that the apparatus should be insulated, because it is much easier to collect electricity from the boiler than from the jet. The steam should not be discharged more rapidly than it is

generated, because running down the pressure always diminishes the electricity in a much greater degree than is due to the mere decrease of density, and frequently changes the electricity of the jet from positive to negative. I find the most convenient way of using my apparatus is to place it out of doors, and to convey the electricity into the house by means of a wire. If, however, the steam were discharged horizontally it might be projected into a chimney, or out of a window, and the objection to using the apparatus within doors would be in a great measure removed.

Dr. Ure, who has witnessed a repetition of most of the experiments I have described, suggested a method of replenishing the boiler without exhausting the steam. He proposed to attach to the boiler a cylindrical vessel, of convenient dimensions, having a stop-cock at the top to receive the water, and another at the bottom to communicate with the boiler. By closing the upper cock, after filling the vessel with water, and then opening the lower one, the water would pass into the boiler without permitting any escape of steam to take place. This arrangement I would certainly recommend to any one who may feel inclined to construct an electro-steam apparatus.

The production of electricity by steam has several important advantages over the common method of obtaining it. An electro-steam apparatus is self-acting, which leaves the operator at perfect liberty to attend to results. Its high temperature renders its action independent of dampness in the atmosphere, which so greatly impairs the energy of an electrifying machine; and finally, its extreme simplicity secures it from injury or derangement.

Wm. Geo. Armstrong.

Newcastle-upon-Tyne, 9th May, 1841.

IV. *On Irish Tin Ore. By* Thomas Weaver, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.**

In a paper drawn up by Aquila Smith, Esq., M.D., *on Irish Tin Ore*, read before the Geological Society of Dublin, on the 9th Dec. 1840, and published in the London, Edinburgh and Dublin Philosophical Magazine and Journal of Science for February 1841, it is stated, "The question has been often asked, is tin ore found in Ireland? and I believe the *only* reply which could be given is, that it was said to have been found in the county of Wicklow, about the year 1796, when the gold mines were worked on account of the Government."

It is also stated, that the existence of tin ore in Ireland was announced for the *first* time in the report of Messrs. Mills,

* Communicated by the Author.

King and Weaver, the directors under Government of the gold workings in that county, dated 1st August 1801, published in the second volume of the Transactions of the Dublin Society; but that in the catalogues of the minerals in Trinity College Museum, drawn up in 1807 and 1818, no specimen of Irish tinstone is mentioned; and hence it is said, "we might almost be induced to suspect that some other substance must have been mistaken for tinstone by the directors of the works." It is then added, "The *next* notice of this metal being found in Ireland is in the catalogue of Irish minerals in the museum of the Royal Dublin Society, published in 1832 by the late Sir Charles Giesecke." And the author concludes with a notice of his examination of some washed sand lately obtained from the gold works now carrying on by a company, under tenure from the Crown, at Croghan Kinshela mountain, accompanied by an analysis of "fragments of an earthy-looking mineral, some of them presenting crystalline planes, abraded by friction," which were contained in the sand; from which analysis Dr. Smith affirms, "in corroboration of the assertion of MM. Mills, King and Weaver, that 'native oxide of tin' exists in the county of Wicklow."

This subject is also adverted to in the able address delivered by James Apjohn, Esq., M.D., as President to the Geological Society of Dublin, on the 10th February 1841, in which it is said, "Dr. Smith, therefore, must be considered as entitled to the credit of removing all doubt as to a point long disputed, and equally interesting to the Irish capitalist and the man of science."

It is a curious subject for reflection, how well-ascertained facts, widely known and never questioned at the time, and afterwards placed repeatedly upon record, should in the lapse of years not only be lost sight of, but that their validity should even become a matter of dispute.

To refresh the memories of the older members of the Dublin Society (among whom my name stands enrolled since the commencement of this century), and to convey information to such persons as may not be cognizant of all the circumstances, it may be useful to enter into some details, and to repeat others, which I certainly never anticipated could have been called for at this time of day.

After the discovery of native gold at Ballinvalley, in Croghan Kinshela mountain, in 1796, and the establishment of the government stream-works there, all the metallic substances disseminated through the diluvial deposit, being concentrated in a mass by the successive operations of washing (well known to professed miners), were always brought under my eye for examination, and it was thus that at Ballinvalley I found that

the gold was constantly accompanied by magnetic ironstone (sometimes in masses of half a hundred weight), by magnetic ironsand, by cubical and dodecahedral iron pyrites; and in small pieces and grains, by specular iron ore, brown and red ironstone, iron ochre, fragments of *tinstone* crystals, wolfram, gray ore of manganese, pieces of quartz and chlorite, and sometimes fragments of quartz crystals. I observed and collected also some specimens, which show that the gold, magnetic ironstone, and wolfram, were each of them frequently intermingled with quartz; and I have also a few specimens which exhibit the gold not only incorporated with iron ochre, but ramifying in slight threads through wolfram.

To satisfy and amuse some of my county of Wicklow friends at the time, I made an assay of the fragmentary crystals of tinstone, showing to them as good metallic tin as was ever produced in Cornwall.

The operations were interrupted by the Irish Rebellion of 1798; but were resumed in 1801, founded on our report made to Government that year. Subsequent researches showed that the gold and the tin ore were not confined to Croghan Kinshela, both being found also in a mountain named Croghan Moira, about seven miles distant from the former mountain. Of the three stream-works established there, one was placed on Ballycreen, in the stream at the eastern foot of the mountain, and minute particles of gold were found accompanied by magnetic ironstone, magnetic ironsand, compact brown ironstone, cubical iron pyrites, and numerous small garnets. In a second, placed in Ballinacapogue brook, on the western side of the mountain, small particles of gold were obtained with magnetic ironstone, magnetic ironsand, and fragments of *tinstone* crystals. A third trial, in Faunanerin stream, on the north-eastern flank of the mountain, did not produce any gold.

The existence of tin ore in the county of Wicklow having been thus well established, the Dublin Society, at the suggestion of their Committee of Mineralogy, offered, among its other annual premiums, in the early part of this century, a premium of *fifty pounds* to the discoverer of a mine of tin ore in Ireland; and a reference to the printed proceedings of that Society will, I think, show that my memory is correct in stating that that premium was offered for several successive years.

In 1818 I drew up my memoir on the Geological Relations of the East of Ireland, which was published in 1819, in the first part of the fifth volume of the Geological Transactions of London. In that memoir I have given a succinct and faithful account of the whole of the proceedings under Government in respect of the gold workings conducted in the

county of Wicklow*, from which I have drawn most of the preceding details. In that memoir I also showed that tin ore was not confined to the county of Wicklow, but that it had been found in the county of Dublin also, though under different circumstances, for which discovery we were indebted to Dr. Thomas Taylor, then living in Dublin, but latterly residing on his property at Dunkerrin Castle, in the county of Kerry. The information on this subject, derived from that gentleman, I have embodied in the 18th section of my memoir (at p. 135 of vol. v. Geol. Trans.), in which, in speaking of the granite region, I have said, "The minerals which I have incidentally observed disseminated in the granite, or imbedded in the contemporaneous veins of granite and quartz that traverse the rock (which more particularly form their native seat), are schorl, tourmaline, garnet, beryl, rock-crystal, epidote, heavy spar, magnetic iron ore, galena, copper pyrites, and iron pyrites. But in addition to these, I am indebted to Dr. Taylor for the information that he has discovered in the granite, malachite, arsenical pyrites, *tinstone*, spodumene, and a new mineral, to which he has given the name of killinite†." And in a note upon the tinstone thus found, I have added, "It is an interesting fact that this metal should at length have been discovered in rock in this country, although found only in a contemporaneous vein traversing a loose block of granite; for its existence in the county of Wicklow, in the form of stream tinstone, had been ascertained several years ago by the operations of the Directors of the works at Croghan Kinshela."

We have it thus upon record that tin ore has been found in *three* distinct localities in Ireland, two in the county of Wicklow, and one in the county of Dublin; the latter being at the south-western foot of Rochetown-hill, adjoining Killiney Bay.

I may here add, that the "tinstone of a hair-brown colour, which accompanies frequently in small grains the native gold found in the streams at Croghan mountain, county of Wicklow," being No. 213 of the late Sir Charles Giesecke's Catalogue of Irish Minerals in the Museum of the Royal Dublin Society, published in 1832, and adverted to by Dr. Smith, was presented by myself before the year 1818.

So lately also as in the year 1835‡, having had occasion to notice a strange misapprehension of facts respecting the gold workings formerly conducted in the county of Wicklow, as

* See §§ 105 to 107 inclusive of the Memoir in pp. 208 to 213 of vol. v. of the Geological Transactions, First Series.

† See the description and analysis of this mineral by my friends Dr. Thomas Taylor and Dr. Francis Barker, Prof. Chem. Trin. Coll. Dublin, in the 13th volume of the Transactions of the Royal Irish Academy, 1818.

‡ See the London and Edinburgh Philosophical Magazine for July 1835.

inserted in the address delivered at the first annual meeting of the Geological Society of Dublin by their President, I then extracted from my memoir on the East of Ireland certain details, in which the occurrence of tin ore in connexion with native gold in both Croghan Kinshela and Croghan Moira mountains is again mentioned.

We have it thus four times in evidence in print, dating from the year 1801 to 1835, that tin ore does occur in Ireland, to which Dr. Smith's testimony in 1840 now adds a fifth; and this independently of what may be found in the printed proceedings of the Dublin Society in the early part of this century. But it follows from the preceding, that Dr. Smith, in his examination of the washed sand lately procured from Croghan Kinshela mountain, has not elicited a single fact that was not established and known more than forty years since.

V. *On the Analogy between the Phænomena of the Electric and Nervous Influences.* *By* MARTYN ROBERTS, *Esq., F.R.S.Ed.**

1. ALTHOUGH much has been said by eminent physiologists in endeavouring to prove that an essential difference exists between the nervous and the electric power, yet there are so many points of analogy untouched by these writers, and the facts they have adduced are so susceptible of explanation by electrical laws, that I feel convinced there is the greatest similarity, if not identity, between the nervous and electric influence, both in their mode of action and in the phænomena produced.

2. In the first place, I may call attention to a beautiful analogy which has, I believe, hitherto escaped observation, viz. the influence of electricity on the circulation of fluids; and there is just cause for believing that a like influence governs the circulation of the blood in the living body.

3. If we calculate the force of the heart, we shall find it wholly inadequate to propel the blood through the fine capillary vessels of the system, unless it be assisted by some other power. There are two sources of resistance to the motion of fluids in tubes, at least two that especially apply to the motion of the blood through the vascular system, viz. the tortuosities of the tubes, and *the adhesion of blood to the sides of the channel through which it is impelled.*

4. The resistance occasioned by the deviation of a channel from a straight line is easily overcome when the motion of the fluid in it is slow; for this resistance is the destruction of the momentum, or tendency of the fluid to move in straight lines,

* Communicated by the Author.

and its amount is the weight of the fluid multiplied by its velocity. It will be found that the blood has but little momentum, for its motion is slow, and that the heart possesses more than sufficient power to overcome all the resistance offered by the tortuosities of the vascular system.

5. The adhesion of a fluid to the sides of the tube through which it passes is a source of resistance that has been much overlooked, but the retardation it would produce in the circulation of the blood would be very considerable were it not compensated or destroyed, especially in the capillary vessels, which are of so small a diameter that their parietes touch every corpuscle of blood that passes them; though some few of the capillaries are large enough to admit two or three globules abreast, many can contain but one red particle of blood in their diameter, while others are so minute as to exclude the red corpuscles, admitting only the fine lymph.

6. The adhesion of fluids to solids is so great, that if every particle of a fluid be in contact with the sides of its channel, a very considerable force will be necessary to overcome the resistance offered to its passage; and this adhesion can be proved in the following manner:—Suspend the solid we wish tried (say a bit of wood) to one end of a fine balance; to the other end add its exact counterbalance in air, then place beneath the wood a vessel containing the liquid, and let the solid rest upon the fluid surface; add weights to the counterbalance until the wood is raised from the liquid, and these weights will give the amount of adhesion. Fig. 1 shows the arrangement. The following is a list of the force of adhesion between a few solids and liquids:—A square inch of wood required 50 grains to detach it from the surface of water; a square inch of gold 446 grains from a surface of mercury; silver, 429 grains; tin, 418 grains; lead, 397 grains; glass, 194 grains.

7. Finding this adhesion to be so great, I concluded it to be the main cause of the resistance offered by capillary tubes to the flow of liquid through them, and that if the adhesion could be destroyed, it would run through such tubes in direct proportion to their diameters; for it is well known that in ordinary cases (where, as I conclude, the adhesion exists) a liquid will not flow through fine tubes in the ratio of their diameter, but in a much less quantity than would be proportionate to larger tubes; indeed, when the channel is capillary, water is either completely arrested or runs in drops at long intervals.

8. Electricity has the power of destroying this adhesion between liquids and solids, for bodies in a like state of electri-

city repel each other; and I found that when a solid resting on a fluid surface was highly electrified, no adhesion took place between them; and the following experiment will show how this also applies to the passage of liquids through tubes.

9. In the bottom of a tin tube (*a*), about an inch in diameter and three or four inches long (fig. 2), fix several metallic capillary tubes (*c*) two or three inches long, opening into the large tube. Now fill the tube (*a*) with water, and suspend it by the wire (*w*) to the prime conductor of an electrical machine. As long as the machine is not in action and the tube and water remain unelectrified, the water will not run through the capillary pipes, but will be completely arrested by the most minute, while through those of a larger diameter it will fall in slowly succeeding drops. But on turning the machine, and thus electrifying the apparatus, the water will instantly run through all the pipes with a rapidity proportionate to the force of the electricity, because the particles of the water and the sides of the pipe being in like states of electricity, repel each other, or at least the electricity overcomes the natural attraction that exists between solids and fluids, and the water is as free to flow as it would be in a tube of such larger diameter that the bulk of liquid contained therein is beyond the sensible attraction or force of adhesion of the pipe.

10. This curious experiment is but a counterpart of what occurs in the living frame; and here, the diameter of the capillary vessels being but little greater than that of the blood corpuscles, the adhesion would act upon every particle of this fluid were it not counteracted by neuro-electric influence, if I may be allowed the term.

11. The force of the heart was calculated from experiment by M. Poiseulle, who ascertained the blood to be propelled with a force that would sustain a column of mercury six inches in height, which is equal to a pressure of three pounds on the square inch, and it is easily proved such a pressure would be insufficient to circulate the blood through a body possessing no living neuro-electric power to overcome adhesion; for let us suppose a proportionate pressure applied to a syringe inserted into the aorta, for the purpose of injecting all the arte-

ries, capillaries, and veins of a dead body; this pressure, on a syringe of one and a half inch in diameter, would be about four pounds; and surely every one will allow that a force of four pounds thus applied would be insufficient to inject the whole body, even with a liquid much less viscid and adherent than blood, and if not sufficient to inject it, how much less to impel such a circulation throughout the system as is maintained in the living being!

12. That the heart derives no assistance from the arteries in propelling the blood, may be proved from the structure of these vessels. To possess a propulsive power they must have muscular contractility; mere elasticity of tissue endows them with no force; it simply causes them to act as regulators to the flow of blood impelled in waves by the rhythmic action of the heart; the air chamber in the fire engine is a similar regulator to the jet of water, compensating for the periodic injections of the pumps; the elasticity of the arteries causes a continuous flow of blood through the capillaries, but cannot force it onwards. It has been fully proved by Müller, Berzelius, Hodgkin and others, that the coats of the arteries are not muscular; and I need not enter into arguments deduced from experiment on their non-muscularity, for I believe this is now the generally acknowledged structure of the arterial coats, and that they possess no more muscular action than so many India-rubber pipes, having but elasticity, which remains long after death, indeed, until decomposition commences.

13. We may then rest satisfied that the arteries in no measure assist the heart to propel the blood, neither can the veins to any considerable amount; and we are therefore driven to the necessity of acknowledging that as the heart's force is insufficient to overcome the adhesion between the blood and the parietes of the capillaries, there must be some power destroying this adhesion, and thus allowing the heart to maintain a free circulation throughout the whole system.

14. This power destroying the adhesion, I hold to be the neuro-electric action. I have already shown that electricity suffices for a like purpose in inorganic bodies, and physiological facts prove the circulation in the capillaries to be greatly, if not altogether influenced by the nerves, which are undoubtedly the sources of the neuro-electric power.

15. The first of these facts is, that the smaller the diameter of the capillaries, the greater is the proportion of nerves distributed upon them, this being necessary to give the larger quantity of *neuro-electric* power requisite to overcome the increased adhesion in these smaller capillaries, caused by the contact of

their parietes with every particle of blood that passes through them; whereas, in those capillaries, where two or three globules can pass simultaneously, a smaller amount of *neuro-electric* influence is required, for the globules are further removed from the parietes of the tube, and therefore less under the influence of the attraction of adhesion.

16. Secondly. Inflammation may be considered as an increased *neuro-electric* action on the capillaries, which electrifying these vessels and the blood to a higher degree than in their healthy state, the adhesion is still further diminished; in those capillaries where red blood before circulated, it now flows in increased quantity; and in those which only admitted lymph in the healthy state, red globules now enter, as may be seen in inflammation of the conjunctiva, &c. The second stage of inflammation is reaction, that is, a diminished amount of neuro-electric influence; the force of adhesion is now uncompensated; the red particles attach themselves to the sides of the vessels, producing the state called congestion.

17. These interesting and successive phænomena can be curiously and beautifully imitated by the apparatus described in section 9, especially if blood (retaining its fluidity) be used instead of water; first the healthy state—a gentle electric action and moderate flow of liquid; then by an increased electric action, imitate the first stage of inflammation with excessive flow of the liquid; then in the state of reaction, which is a deficiency of electricity, we have a cessation of the jet of liquid, with the passage of a few drops at intervals; this imitates congestion; and it is but to transfer these successive actions to the living body, and the analogy is complete.

18. Again, what is turgescence but the result of an increased neuro-electric action, which, destroying the adhesion of the blood to the minute capillaries of the part, renders the force of the circulation sufficient to propel the blood into the before empty cavities? It can be nothing else but this, for the power of the heart is constant, while turgescence is but an occasional phænomenon; neither could the heart direct an impulse to the blood towards any particular part of the system; nor do I see that any theory of this peculiar action, except the one I have ventured to propound, will suffice to explain it; whereas the destruction of the force of adhesion, which I have fully shown to be not only possible, but most probable, is quite sufficient to account for all the phænomena, and will, I trust, be found a simple and satisfactory explanation.

19. Inflammation or turgescence cannot take place without a change of nervous action, and there are many proofs of these phænomena being entirely dependent upon the nerves leading

to the parts affected; thus it has been found that no inflammation ensues on the application of irritants to the eye, if the fifth nerve expanded upon the conjunctiva be divided; the application of ammonia to an eye thus injured produced no effect, while on the other eye of the animal, the nerves of which remained untouched, a similar application produced a violent inflammation.

20. It has also been proved that division of the pudic nerve completely arrested the power of producing turgescence in the singular structure of the corpora cavernosa, discovered by Müller.

21. The phænomena of blushing can be easily explained if we allow that an affection of the mind produces an increased neuro-electric action in the face and neck, which by compensating for the resistance offered by the capillaries in these parts to the flow of blood, they are easily injected by the force of the heart, and that suffused appearance called blushing is the result.

22. The languid circulation in paralysed limbs is the effect of a deficiency of neuro-electric action; there is a greater resistance offered to the passage of blood through the paralysed capillaries, a slow and difficult circulation ensues, and when this happens to excess, those vessels that before admitted the red globules, now only receive lymph, and a paleness of the part is the consequence, with diminished nutrition.

23. These several phænomena tend to prove that a change of nervous action in the capillaries is sufficient to cause an increased or diminished circulation of blood through them, and I have shown this may be accomplished in inorganic bodies by a change in their electric condition, and I trust the analogy has been proved to be complete between this and the nervous or neuro-electric action in the living body. Were any thing wanting to confirm the present hypothesis, I may add, that microscopic observation shows us the red globules of blood passing through the capillaries in a state of extreme self-repulsion, and also removed from the sides of these tubes.

24. I anticipate, with some degree of satisfaction and confidence, that the new theory I have here ventured to propound of the forces tending to motion of the blood, will have the effect of increasing our pathological knowledge of the various changes in the circulation, and clear up many of those obscurities that have long retarded the progress of medical science.

25. The question may be asked, whence is this electricity or the neuro-electric power of the body derived? I answer, by respiration from the atmosphere; for oxygen gas, in com-

bining with either liquids or solids, always sets electricity free, and in the process of respiration a large quantity of oxygen hourly combines with the blood or its carbon; and there is necessarily an immense quantity of electricity evolved during this action, which, electrifying the blood, is sufficient to produce all the phænomena I have explained. How far this electricity may be attracted and retained by the brain and spinal chord, may form the subject of subsequent inquiry; but there can be little doubt that the electricity of the living frame is thus derived from the atmosphere, and it is extremely probable that the air expired from the lungs will be found by experiment to be highly electrified from the same cause.

26. The fact of the annihilation, by electricity, of the adhesion between solids and liquids, will be found of great value to the practical anatomist in making injections of anatomical preparations.

27. If the part to be injected is strongly electrified while the coloured liquid is forced into it, the great resistance that in general exists to the passage of the liquid through the fine capillaries will be overcome, the fluid will pervade the whole tissue, and the minutest capillaries will be filled in a manner hitherto deemed impossible.

28. The air-pump is sometimes used to assist the injection of anatomical preparations, and it may be employed in conjunction with electricity; but I believe electricity will be found sufficient without such aid, and I will endeavour to describe the simplest method of thus injecting a part.

29. Suspend a metallic plate from the ceiling, or any other distant support, by three silken strings, in the manner the pan of a balance is hung; upon this is to be placed the part to be injected with the reservoir of liquid, in order that it may be well insulated, and thus fitted to receive a strong electric charge. The liquid for injection may be contained in an elastic bottle and subjected to a constant pressure by a weight, or it may be in a vessel together with compressed air, the expansion of which will force out the liquid when the channel is open; or in a reservoir, at a height above the part to be injected, and communicating with it by a pipe. These arrangements I merely throw out as suggestions; an ingenious person will soon contrive his best method of forcing in the liquid. I only impress the necessity of insulating all the apparatus, together with the part to be injected, otherwise it cannot become charged with electricity: the best mode of insulating is to place the whole upon the metallic plate, suspended upon dry silken strings three or four feet long.

30. When the apparatus, &c. is upon the plate, make a me-

tallic communication between it and the prime conductor of an electric machine, then charge the whole powerfully with electricity, and the liquid will now flow through the minutest capillaries, and this without any risk of rupturing these fine vessels.

[To be continued.]

VI. *On the Action of Peroxide of Lead on Cinnamic Acid and on Salicine. By* John Stenhouse, *Esq.**

IT is well known that when cinnamic acid is digested with nitric acid, oil of bitter almonds is formed, which is easily recognisable by its smell, and the cinnamic acid is converted into the benzoic acid. If strong nitric acid is employed, however, and the digestion is long continued, the benzoic acid is still further resolved into the nitro-benzic acid of Mulder. I was induced to try if peroxide of lead would also have the effect of converting cinnamic into benzoic acid. On boiling a solution of cinnamic acid with peroxide of lead, the smell of oil of bitter almonds was immediately emitted, and the peroxide of lead became of a light ochre colour. This change of colour was owing to the reduction of the peroxide of lead to the state of protoxide. In order to determine with what acid the lead had combined, the salt was decomposed with dilute sulphuric acid, and the organic acid thus set free was purified by repeated crystallizations. It had all the external characters of benzoic acid. The following are the results of its analysis:—

gramme.

I. 0·2879 substance gave 0·725 carbonic acid, and 0·1320 water.

II. 0·2885 substance gave 0·7295 carbonic acid, and 0·129 water.

	I.	II.	Calculated numbers.
C	69·62	69·91	69·27
H	5·07	5·00	4·84
O	25·31	25·09	25·89
	100·00	100·00	

Now these approach pretty nearly the calculated numbers for hydrated benzoic acid, the formula for which is

$$\begin{aligned} C\ 14 &= 69{\cdot}27 \text{ per cent.} \\ H\ 6 &= 4{\cdot}84 \\ O\ 4 &= 25{\cdot}89 \end{aligned}$$

* Communicated by the Author.

The slight excess in the carbon is probably owing to a little of the cinnamic acid having escaped decomposition.

Peroxide of lead is therefore a more elegant test for cinnamic acid than nitric acid, as there are no fumes, as is the case when nitric acid is employed to disguise the smell of oil of bitter almonds, the taste of which is easily perceptible in the liquid.

Hypochlorite of lime also converts cinnamic acid into benzoic, but the oily liquid which is then formed is not oil of bitter almonds, as its taste and smell are quite different, and much more aromatic. The extremely small quantity in which it was obtained, however, unfortunately prevented me from being able to subject it to examination. The cinnamic acid on which I operated, was obtained by boiling balsam of Peru with a solution of potash, which is the most œconomical process for cinnamic acid.

Action of Peroxide of Lead on Salicine.

When a solution of salicine was boiled with a quantity of peroxide of lead, the peroxide of lead gradually assumed a light brown colour. The whole was then thrown upon a filter, and the clear liquid which passed through had a bitter, and at the same time a sweetish taste, which reminded one at once of that of salicine and of the soluble salts of lead. A portion of the liquid was introduced into a retort, and a little sulphuric acid was added. This occasioned a copious precipitate of sulphate of lead, and the red coloured substance called rutiline, which is always produced when sulphuric acid acts upon salicine, also made its appearance. On applying a gentle heat the liquid which passed into the receiver was strongly acid, and when heated with a few drops of nitrate of silver, the oxide of silver was reduced to the metallic state, clearly indicating the presence of formic acid. The other portion of the liquid, on standing some time, became muddy, and deposited a small quantity of a flocculent precipitate. On adding a few drops of ammonia, however, a very bulky precipitate fell, which was soluble in acetic acid and in solution of potash. It consisted of the combination of lead and salicine discovered and analysed by Piria. On treating it with a current of sulphuretted hydrogen till all the lead was precipitated, and purifying the salicine thus set free by two or three crystallizations, I found that the salicine had undergone no alteration. On being subjected to analysis,

gramme.

I. 0·3247 substance gave 0·65 carbonic acid, and 0·1865 water.

The formula for salicine is $C_{42}\,H_{29}\,O_{22}$ =

		Calculated numbers.
C	55·35 per cent.	C_{42} = 55·69
H	6·38	H_{29} = 6·26
O	38·27	O_{22} = 38·05
	100·00	100·00

It appears then that when salicine is treated with peroxide of lead, formate of lead and the combination of lead and salicine are the results.

Salicone.—When salicine is intimately mixed with half its weight of quicklime and subjected to destructive distillation, a quantity of a heavy reddish coloured oil and some water pass into the receiver, while carbonate of lime, mixed with charcoal, remains in the retort. The oil has a very pungent taste, and an empyreumatic smell, somewhat resembling that of creasote. It was rendered colourless by one or two rectifications which deprived it of most of its empyreumatic odour. Alkalies in the cold had no effect upon it, but when heat was applied the oil was changed into a dark resinous mass. Nitric acid attacked it violently, even in the cold, with the evolution of deutoxide of azote. Sulphuric acid reddened it in the cold, and when assisted by heat blackened it, and gave off sulphurous acid. It dissolved iodine very readily, but without explosion, even when heat was applied. When rectified the first and last portions of the oil were rejected. The middle portion had a specific gravity of 1·0212. It began to boil at 260° Fahrenheit, but the boiling point gradually rose to 272° Fahrenheit. It was rendered anhydrous by being allowed to stand for some days over fused chloride of calcium. When subjected to analysis,

gramme.

I. 0·308 substance gave 0·842 carbonic acid, and 0·214 water.

II. 0·3085 substance gave 0·849 carbonic acid, and 0·206 water.

III. 0·326 substance gave 0·886 carbonic acid, and 0·228 water.

	I.	II.	III.
C	75·59	76·12	75·45
H	7·72	7·41	7·77
O	16·69	16·47	16·78
	100·00	100·00	100·00

It is somewhat remarkable that this is identical with the composition which Dr. Ettling found for creasote.

Analysis of creasote by Ettling.

Carbon	75·56
Hydrogen...............	7·78
Oxygen..................	16·66
	—— 100·00

As, however, I have not been able to determine the atomic weight of salicone, I do not think it worth while deducing any formula from these numbers. When salicine is destructively distilled without any lime, it also yields an empyreumatic oil.

Glasgow, April 10, 1841.

VII. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from vol. xviii., p. 546.]

Action of High Temperature on Mellitate of Ammonia. By F. Wöhler.

It might with some probability be supposed that the honey-stone or mellite is contained in the mass of the brown coal of Artern in such small particles as to escape observation. According to Wöhler's experiments, however, this is not the case. The substance which remains after the mineral has been treated with carbonate of ammonia is alumina; but it always contains a little mellitic acid, probably as basic salt, for on dissolving in nitric acid and evaporation, small crystals are obtained, which are regenerated mellite, $\underline{\dddot{Al}}\ \overline{Me}^3 + 18$ aq. Mellitate of ammonia loses ammonia by boiling, and is converted into an acid salt; if, therefore, the mineral has been boiled with the carbonate, it may easily happen that an acid salt is formed, and thereby alumina dissolved. This alumina then passes into the lead or silver salts, from which the acid is then obtained in an impure state, viz. containing alumina. It is therefore better, on decomposing the mellite with carbonate of ammonia, to add a few drops of caustic ammonia from time to time. The acid ammonia salt is much more soluble than the neutral one. The acid crystallizes from a concentrated solution in fine silky needles; does not lose water when heated to 200° C. Formula, $C^4\ O^3 + H^2\ O$. Wöhler formerly stated that nitric acid precipitated a somewhat insoluble salt from the neutral mellitate of potassa; this is, however, not

the acid salt, but a compound of the acid mellitate with nitrate of potassa, $\dot{K}\,\underline{\overset{\therefore}{N}} + 4\,\dot{K}\,\overline{Me}^2 + 10$ aq. By heating it loses 7 per cent. water, or 6 atoms; the other 4 atoms are not driven off before the salt decomposes; its rational formula is, therefore, $\dot{K}\,\underline{\overset{\therefore}{N}} + 4\,(\dot{K}\,\overline{Me} + \underline{\dot{H}}\,\overline{Me}) + 6$ aq. The true acid mellitate of potassa forms large transparent crystals, easily soluble $(\dot{K}\,\overline{Me} + \underline{\dot{H}}\,\overline{Me}) + 4$ aq. $\frac{4}{5}$ths of the water is driven off by heat. Mellitate of ammonia ($N^2\,H^8\,O + \overline{Me}$), when heated to 150° C, loses ammonia and water, and is converted into two new substances, both of which contain nitrogen. One of them is an acid, and this is probably the first case known of the production of an organic acid containing nitrogen from the ammonia salt of one which contains no nitrogen. The metamorphosis may be easily produced by exposing a thin layer of the mellitate for several hours to a temperature of 150° to 160° in an oil-bath. The operation must be continued until no more ammonia is evolved. The temperature must not rise above 160°. (A concentrated solution of mellitate of ammonia, if contained in a sealed tube, may be exposed for hours to a temperature of 200° without any change being effected.) The pale yellow powder which remains is treated with *cold* water, which separates it into two substances; it is washed with cold water as long as the filtered solution reacts acid. On evaporating an acid salt is obtained as a scarcely crystalline mass. The insoluble body is called paramid, the acid in the soluble ammonia salt, euchronic acid (εὔχροος, beautiful colour). Paramid is white, and forms hard conglomerated masses; becomes yellow in the air; perfectly tasteless and inodorous; when moistened looks and smells like wet clay; insoluble in water, alcohol, nitric, and even nitro-muriatic acid; is dissolved by hot sulphuric acid, but is separated unchanged by water. Heated to 200° undergoes no change; does not lose water. When heated more strongly is decomposed, gives off cyanide of ammonium, and forms a sublimate which is partly bluish-green and half fused, and consists partly of sulphur yellow crystals of a very bitter taste. Paramid is dissolved by long (several days) boiling with water, and on evaporating an ammonia salt is obtained. At a temperature of 200° this change is soon effected. The salt produced is *acid mellitate of ammonia*. Hence this body belongs to the class of the amides. The composition of paramid is $C^8\,H^2\,N^2\,O^4$. Two atoms of mellitate of ammonia lose 1 atom of ammonia and 4 atoms of water, $C^8\,H^{16}\,N^4 O^8$, or 2 (N^2

$H^8 O, C^4 O^3) - (N^2 H^6 + H^8 O^4) = C^8 H^2 N^2 O^4$. However, the process is not simple, on account of the simultaneous formation of euchronic acid. By boiling with water paramid takes up 2 atoms and forms 1 atom ammonia and 2 atoms of mellitic acid, $C^8 H^2 N^2 O^4 + H^4 O^2 = 2 C^4 O^3 + N^2 H^6$.

The metamorphosis takes place best at 200°; at 100° euchronate of ammonia is formed, and at this temperature remains unchanged. Paramid is soluble in alkalies, and when heated with them becomes voluminous, and is dissolved on addition of water. If hydrochloric acid be instantly added, unchanged paramid is precipitated; if, however, it be allowed to stand, ammonia is evolved, and the solution then contains at first euchronic acid, and at a later period mellitic acid. This change takes place instantly on the application of heat. By dissolving paramid in ammonia and instantly precipitating by nitrate of silver, a compound of paramid with oxide of silver was obtained, which did not lose water at 150°; this shows that paramid does not contain water, and cannot therefore be $C^8 N^2 O^3 + H^2 O$.

Euchronic Acid.—That part of the product obtained by heating mellitate of ammonia, which is soluble in water, is acid euchronate of ammonia; if the decomposition be not completed it contains acid mellitate; if the temperature is too high it is yellow, and contains the yellow bitter matter. It is deposited in white scarcely crystalline crusts, reacts strongly acid, is not very soluble in cold water, and the paramid must therefore be edulcorated for a long time. To obtain the acid, the salt is dissolved in the smallest possible quantity of hot water, and the hot solution treated with nitric or hydrochloric acid; on cooling the acid separates as a white crystalline powder. It may be purified by re-solution in boiling water, and may be obtained in small crystals. It is very strongly acid, tastes like bitartrate of potassa. The crystals lose water when heated, and become opake. If crystallized from a solution containing sal-ammoniac, it often contains a trace of ammonia, and is then of a yellowish colour. It loses 2 atoms of water at 200°. Can be heated to 280° without change; further heated is decomposed, and gives cyanide of ammonium and a deep green bitter sublimate. Heated with but very little water to 200° in a closed tube it is dissolved, and in the solution is found acid mellitate of ammonia. If a piece of metallic zinc be put into a solution of euchronic acid, it becomes instantly covered with a layer of a splendid blue colour; if the plate of metal be dipped into dilute hydrochloric acid, the blue deposit is separated, and may be washed and dried.

It then forms a black mass, which does not contain zinc. On heating very slightly, even on paper, it becomes instantly white, and is changed into euchronic acid. This is exactly the same as with the deoxidized indigo, only *vice versâ* with regard to the colours. Wöhler therefore proposes for this blue body the name of Euchron. It is soluble in ammonia and the alkalies, yielding splendid purple colour; but the solution decolorated with great rapidity. If a solution of protochloride of iron be mixed with a solution of euchronic acid no change is produced, but on addition of an alkali a voluminous violet-coloured precipitate is produced, which oxidizes in the air with the greatest rapidity. By the action of zinc on a boiling solution of euchronic acid an almost imperceptible quantity of gas (hydrogen or ammonia) is evolved, but this is doubtless a secondary product; a white powder is formed in small quantity at the same time; it appears to be mellitate of zinc. A hot solution of euchronic acid forms, in a dilute solution of nitrate of silver, a yellow precipitate, which is dissolved again on shaking, but on cooling is deposited. It is not dissolved by ammonia, but becomes changed and passes milky through the filter, &c. The lead salt is obtained by adding a boiling solution of euchronic acid to a dilute solution of neutral acetate of lead. The salt is deposited on cooling as a yellow powder, or in small yellow crystals. The filtered fluid, if boiled, deposits white mellitate of lead. Heated to 160° loses 11·36 per cent. water. Both this salt (dried at 160°) and the silver salt, however, still contain water. The composition of the euchronic acid is $C^{12} N^2 O^6$. Atomic weight 1694·29. The anhydrous silver salt is $2 \dot{Ag} + C^{12} N^2 O^6$, the hydrated $2 \dot{Ag} + C^{12} N^2 O^5 + H^2 O$. The lead salt, dried at 150°, is $\dot{Pb} + C^{12} N^2 O^6 + H^2 O$. The crystallized salt is $\dot{Pb} + C^{12} N^2 O^6 + 5 H^2 O$. Euchronic acid heated to 200° is $C^{12} N^2 O^5 + 2 H^2 O$; the crystallized contains 2 atoms of water more. The acid euchronate of ammonia, obtained by heating the mellitate, is $N^2 H^8 O + H^2 O . C^{12} N^2 O^6$; it is not changed at 200°. The decomposition of the mellitate of ammonia is then as follows:—Seven atoms are decomposed and form 2 atoms of paramid, 1 atom of euchronic acid, 4 atoms of ammonia, and 14 atoms of water.

2 atoms Paramid	=	C^{16}	H^4	N^4	O^8
1 atom Euchronic acid . .	=	C^{12}		N^2	O^6
4 atoms Ammonia	=		H^{24}	N^8	
14 atoms Water	=		H^{28}		O^{14}
7 atoms Mellitate of ammonia	=	C^{28}	H^{56}	N^{14}	O^{28}

By the metamorphosis of euchronic acid by water at 200°,

into mellitic acid and ammonia, 1 atom of acid and 3 atoms of water form 3 atoms of mellitic acid and 1 atom of ammonia.

$$\begin{array}{ll} 3 \text{ atoms } \overline{\text{Me}} = & C^{12} \quad\quad O^9 \\ 1 \text{ atom } \text{Am} = & \quad H^6 N^2 \\ \hline & C^{12} H^6 N^2 O^9 = C^{12} N^2 O^6 + 3 H^2 O. \end{array}$$

By the conversion of paramid into mellitate of ammonia, only two atoms of mellitic acid are formed. Moreover, 3 atoms of paramid = 2 atoms euchronic acid + 1 atom ammonia. Paramid is bieuchronate of ammonia minus 2 atoms of water. It is probable that paramid and euchronic acid form in high temperatures other interesting products. A deep green sublimate and yellow needles are always formed, &c. &c. &c.—(*Annalen der Pharmacie*, xxxvii. 63.)

Products of the Action of Nitric Acid on Naphthalin. By C. de Marignac, *of Geneva.*

Laurent has shown that by the action of nitric acid on naphthalin two products are obtained; when the action takes place in the cold a substance is formed having the composition $C^{20} H^{14} N^2 O^4$. Nitronaphthalase (Berzelius's nitrite of the oxide of Ikodekatesseryl), by the aid of heat, $C^{20} H^{12} N^4 O^3 = 2 (C^{10} H^6 . N^2 O^4)$. Nitronaphthalese (nitrite of the oxide of Dekahexyl) is produced. The action of nitric acid does not, however, cease here; if it be continued two other substances are formed. When nitronaphthalese is once formed, the acid acts but very slightly, and no more red vapours are evolved until the greater part of the nitric acid has passed over, when the temperature rises and red vapours are formed. In order to obtain the products thus formed, De Marignac added the nitric acid to the naphthalin in the retort in small portions, and waited each time until all action had ceased; the operation was continued several days, and the residue in the retort every evening washed with water, in order to extract soluble matters. Three products were thus obtained:—1st, An acid, nitronaphthalic acid, soluble in water. 2ndly, An insoluble residue, nitronaphthalise. 3rdly, A substance in fine flexible needles, which sublimed in the neck of the retort and passed over dissolved in the nitric acid; this substance proved to be nitronaphthalese.

Nitronaphthalise.—The residue in the retort must be washed with boiling water and then treated with cold æther, which dissolves a little of a yellow resinous substance. The nitronaphthalise thus obtained has a tinge of yellow, is almost insoluble in æther, even when boiling, but little soluble in boiling alcohol, out of which it falls as a crystalline powder, in-

soluble in cold water; boiling water dissolves merely a trace; slightly soluble in nitric acid, precipitated by water. Caustic and carbonated alkalies dissolve it with a red colour, which soon becomes black; melts a little above 100°, may be volatilized without residue, but is often rapidly decomposed with slight detonation. Formula, $C^{20}\ H^{10}\ N^{6}\ O^{12}$, or, according to Laurent's view, $C^{20}\ H^{10}\ O^{3} + 3\ N^{2}\ O^{3}$. The alkaline solution is at first red, becomes brown, and ammonia is evolved; on the addition of an acid, carbonic acid is given off, and brown flocks are precipitated. This brown body is insoluble in water, alcohol and æther, soluble in caustic and carbonated alkalies, these solutions form brown precipitates in silver, lead, baryta, and lime salts; the compounds thus obtained have no determinate composition. The probable composition of the brown body appears to be $C^{12}\ H^{6}\ N^{2}\ O^{5}$. By the formation of this brown substance, formic and hydrocyanic acids are also probably produced.

Nitronaphthalic Acid.—Is formed in very small quantities under peculiar circumstances. It is generally mixed in its solution with a small quantity of a resin, from which it may be separated by evaporating to dryness and treating with a little cold water; the little nitric acid which remains suffices to dissolve the resin, and nitronaphthalic acid is left behind as a yellow crystalline powder. May be purified by re-solution in water or æther, and crystallization. The resin thus separated still contains nitronaphthalic acid. This acid is soluble in boiling water, and more so in alcohol and æther; does not contain water. Formula, $C^{16}\ H^{10}\ N^{2}\ O^{12}$. This is the crystallized acid; in the baryta and silver salts two atoms of water are replaced by two atoms of base. The formula of the silver salt is, therefore, $C^{16}\ H^{6}\ N^{2}\ O^{10} + 2\ Ag\ O$. The ammonia salt is easily soluble in water, and tolerably so in alcohol; the silver salt is white and insoluble. The ammonia salt and acetate of lead give a basic lead salt, which is a yellowish white insoluble powder, $C^{16}\ H^{6}\ N^{2}\ O^{10} + 4\ Pb\ O$. The baryta salt is yellowish white, insoluble, contains no water. The nitronaphthalic acid is, therefore, evidently a bibasic acid. Nitronaphthalate of lead seems to form, under the action of sulphuretted hydrogen, another acid, which M. de Marignac intends at a future period to examine.

In a second paper M. de Marignac states, that by the action of nitric acid on hydrochlorate of chloronaphthalese, $C^{20}\ H^{16}\ Cl^{8}$, he obtained an acid and a volatile fluid. The acid did not agree at all with Laurent's naphthalic acid. The fluid product passes over into the receiver with the nitric acid, and separates from it on account of its greater specific gravity;

part, however, remains dissolved. It is purified by washing and distillation with water. It is perfectly colourless, transparent, sp. gr. = 1·685 at 15° R. (?); has an irritating smell like chloride of cyanogen; is scarcely soluble in water; easily so in alcohol and æther. Forms, by the action of an alcoholic solution of potassa, a crystalline potassa salt. Formula, $C^1 Cl^2 N^2 O^4$, perhaps $C Cl^2 + N^2 O^4$. The density of the vapour could not be determined.

M. de Marignac considers naphthalin as composed of two hydrocarbons, $C^{16} H^8 + C^4 H^8$; chlorine can only extract one half of the hydrogen and form $C^{16} H^8 + C^4 Cl^8$; it is evident that eight atoms must be in a different state of combination from that in which the other eight atoms are. These two bodies may be separated from each other, but they then enter into new combinations. Nitronaphthalic acid may be considered as an oxide of $C^{16} H^6 N^2 O^4$, viz. $C^{16} H^6 N^2 O^4 + O^6$. This radical has arisen from $C^{16} H^8$ by the substitution of one equivalent hyponitric acid for one equivalent hydrogen. Hydrochlorate of chloronaphthalese is $C^{16} H^8 + C^4 H^8 Cl^8$. From the radical $C^{16} H^8$ the naphthalic acid is derived, while, from the other, $C^4 Cl^8 + N^8 O^{16}$ is produced, &c.

Action of Potassa on Camphor. By Delalande.

When the vapours of camphor are passed over a mixture of potassa and lime, heated in a tube to between 300° and 400°, they are absorbed, and no gas is evolved; the product is extracted with hot water, and the filtered solution precipitated by an acid; a white crystalline acid is thus obtained, which, when pure, may be distilled unchanged, and for which the name of Campholic acid is proposed. Campholic acid crystallizes well out of its alcoholic solution. It has a very similar appearance to camphor; reddens slightly litmus paper, but saturates bases perfectly; melts at 80°; boils at 250°; not soluble in water, to which, however, it imparts an aromatic odour; readily soluble in alcohol and æther. The temperature used during preparation must be strictly attended to, for the operation does not always succeed. It is best performed in closed tubes; the vapours are passed backwards and forwards over the heated mixture several times. The formula is $C^{20} H^{36} O^4$. Campholate of silver is obtained by precipitating nitrate of silver with campholate of ammonia; is a white caseous substance, which easily retains nitrate of silver, and must be dried, pulverized, and repeatedly washed. Consists of $C^{20} H^{34} O^3 + Ag\,O$. Campholate of lime, when an ammoniacal solution of campholic acid is added boiling hot to chloride of calcium. It is white, crystalline; more readily soluble in cold

than in hot water, $C^{20} H^{36} O^{4}$, Ca O, or $C^{20} H^{34} O^{3}$, Ca O + H^{2} O. The specific gravity of the vapour of campholic acid is 6·058, according to experiment; calculated, it is 5·938. Campholic acid is, camphor + 2 Aq.

If campholic acid be distilled with anhydrous phosphoric acid, and the product rectified, a colourless oil is obtained, which boils at 135°. Its formula is $C^{18} H^{32}$. It is named Campholen. The specific gravity of the vapour is 4·353, calculated 4·344.

By the distillation of a mixture of campholic acid with caustic lime, an oil is obtained, whose formula is $C^{19} H^{34}$ O, i. e. anhydrous campholic acid, minus carbonic acid, $C^{20} H^{34} O^{3} - C O^{2}$. (*Annales de Chimie et de Phys.* trois. ser. i. p. 120—127.)

On Bromic Acid, and its Salts.

Notwithstanding the various researches which have been made on bromine since its discovery by Balard in 1826, several points in the history of this interesting body have hitherto been but imperfectly investigated, and among others, more especially its combinations with oxygen. To fill up some of these voids has been the object of Dr. Rammelsberg, who, in a paper recently laid before the Academy of Berlin, has described a number of the bromates, and several experiments to obtain hyperbromic acid, in which, however, he has not proved successful. Balard's experiments to obtain hypobromous acid showed that the tendency of bromine to combine with oxygen was exceedingly weak; bromic acid decomposes, under all circumstances, more easily almost than chloric acid, and in this respect cannot at all be compared with iodic acid. Bromate of potash is converted, on heating below strong redness, immediately into bromide of potassium; gaseous chlorine does not produce at any temperature, even with an excess of base, any decomposition. Bromic acid itself decomposes at a temperature of 120° into bromine and oxygen. The iodates of barytes, strontian and lime are converted on heating into basic hyperiodates; such is not the case with the corresponding bromates, which are immediately reduced to the state of bromides. Among the salts examined, the bromate of ammonia is remarkable from the property, not only on heating, but even after a short time, without any external cause, of decomposing with violent detonation into bromine, nitrogen and water; while probably at the same time oxygen is set free, or forms an oxide of nitrogen. The bromate of potash, of soda, and of the oxide of silver, are anhydrous; the two first crystallize in forms of the regular system, the latter is a pulverulent body of difficult solution. The salts of barytes, strontian lime, and lead, contain one atom of water; the ba-

rytes and lime salts cannot be obtained well crystallized; but the other two are isomorphous, which is likewise the case with the zinc and magnesia salts, both of which contain six atoms of water, and crystallize in regular octahedrons. The copper salt contains five atoms of water. The bromate of the protoxide of manganese decomposes soon after its formation, bromine being set free, and peroxide of manganese deposited. Several of these bromates combine with ammonia in the moist way, which was not known before. The copper and silver salts take up each two equivalents, the zinc salt one equivalent of ammonia, and, moreover, three atoms of water. Connected with these experiments is the discovery of a double salt of the iodide and bromide of mercury, which is formed direct, and consists of an equal number of atoms of both salts. (*Bericht der Akademie zu Berlin*, Dec. 1840.)

Composition of Guano.

The following analysis of this interesting and important substance, made by M. Voelckel in the laboratory of Prof. Wöhler, confirms what Klaproth found*, viz. that the guano, among its characteristic constituents, contains, besides unchanged uric acid, a considerable quantity of two of its usual products of decomposition, viz. oxalic acid and ammonia. 100 parts of moist guano contain:—

	Voelckel.		Klaproth.
Urate of ammonia	9·0		16·0
Oxalate of ammonia	10·6		
Oxalate of lime	7·0		12·75
Phosphate of ammonia	6·0		
Phosphate of ammonia and magnesia	2·6		
Sulphate of potash	5·5		
Sulphate of soda	3·8	common salt	0·5
Chloride of ammonium	4·2		
Phosphate of lime	14·3		10·0
Clay and sand	4·7		32·0
Undetermined organic substances, of which about 12 per cent. is soluble in water. A small quantity of a soluble salt of iron. Water	32·3		28·75
	100·0		100·00

(*Ann. der Chem. und Pharm.* vol. xxxviii. part 3.)

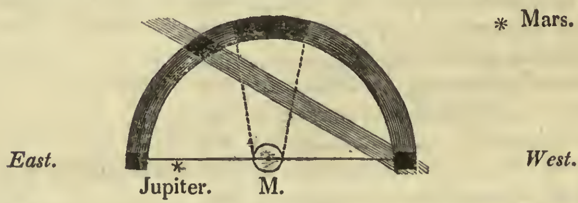
* A memoir on Guano, by Laugier, will be found in Phil. Mag., First Series, vol. xxiv. p. 126; from which it appears that Fourcroy and Vauquelin obtained similar results. Edit.

VIII. *On a Paraselene witnessed on May 6th, 1841. By* W. R. Birt, *Esq., Librarian and Assistant Secretary to the Metropolitan Institution.*

To E. W. Brayley, Jun., Esq.

My dear Sir,

I HAD an opportunity last night of witnessing an interesting exhibition of paraselene. The halo occurred in a modification of *cirro-stratus*, which I have termed *nebula suspensa*, in order to distinguish it from other varieties of *cirro-stratus*. I believe it is the only variety that exhibits halos; I do not recollect at the moment having observed them in others. One half only of the halo was visible, the moon being low at the time of observation; this half exhibited three portions, which were more strongly illuminated than the rest of the semicircle. The inclosed rough sketch will convey some idea of the position of these illuminated portions; the semicircle was very distinct, the base or the diameter joining the two extremities and passing through the moon, being parallel with the horizon. Both these extremities were strongly illuminated as compared with the semicircle generally, but faint as compared with the moon; these illuminated portions were not well defined; their appearance did not convey to me the idea of their being images of the moon, but merely portions of the halo more strongly illuminated than the rest. Perhaps this little figure will give the best idea of their appearance.



The darkened portions represent the illuminated portions of the semicircle.

The illuminated portion at the summit of the semicircle was very interesting; it was fainter than the portions above mentioned, and filled a small segment of the halo, in the same manner as a pencil of rays, diverging from a luminous point, would illuminate the segment of a ring situated within its cone. This illuminated portion appeared somewhat of a lozenge form, having the part immediately over the moon brightest and broadest; it became gradually fainter and narrower towards its eastern and western extremities. The difference between it and the illuminated extremities of the semicircle was very distinct.

I first saw this interesting phænomenon about a $\frac{1}{4}$ past 11, and as I continued to watch it, several appearances presented themselves which (should they have been noticed by others) may probably assist in determining some interesting particulars relative to the clouds that were passing at the times of observation, the area they extended over, etc. I therefore subjoin the observations I made until the disappearance of the paraselene;

And remain, my dear sir, yours very truly,

W. R. Birt.

Metropolitan Institution, May 7, 1841.

1841. *Mean Time estimated to the nearest minute.*

May 6. $11^h 45^m$.—The western illuminated portion nearly disappeared, very faint, the semicircle still visible.

May 6. $11^h 49^m$.—The western portion again visible, with a streak of cloud extending from it to the east of the summit of the semicircle.—The portion of the semicircle, where the streak of cloud intersected it, was illuminated similar to the three portions above mentioned.

This streak of cloud passed over towards the south-east, the eastern extremity of the semicircle being at the time much more strongly illuminated than the western.

May 6. $12^h 2^m$.—A general cloudy appearance westward of the semicircle, and the western extremity very distinct, approaching a decidedly circular form.

May 6. $12^h 5^m$.—The illumination of the eastern extremity has nearly disappeared.

May 6. $12^h 8^m$.—Mars and Arcturus perfectly free from clouds.

May 6. $12^h 12^m$.—A splendid meteor passed near the semicircle directly towards the horizon in the clear part of the heavens.

The clouds passing over appear to be curled cirri; those portions of them that intersect the semicircle in their passage are more illuminated than the others.

May 6. $12^h 15^m$.—The illuminated summit of the semicircle has entirely disappeared, owing to the passage of the clouds towards the south-east; the extremities of the semicircle still faintly visible.

May 6. $12^h 18^m$.—The semicircle obliterated, a small segment only visible to the east, with the eastern extremity very faint.

The degree of illumination was very variable, particularly towards the end of the observations; the western extremity was just discernible when I left observing.

IX. *On the Polarization of the Chemical Rays of Light.* *By* JOHN SUTHERLAND, *M.D., Liverpool**.

IT has been long known that the invisible rays of the solar light, which manifest their presence by inducing chemical action, are possessed of some of the properties of the luminous rays. Their capability of being reflected and refracted must have been observed at the time of their discovery; and Dr. Thomas Young proved that they were capable of producing the phænomena of interference, by allowing the rays beyond the violet extremity of the spectrum to fall on paper covered with chloride of silver, after having been transmitted through glasses showing Newton's rings. The same phænomenon was also exhibited directly by M. Arago, who made use of Fresnel's experiment for the purpose of demonstrating it.

On the 21st December 1812, M. J. E. Bérard read a paper before the French Institute, "Sur les propriétés des différentes espèces de rayons qu'on peut séparer au moyen du prisme de la lumière solaire," which was published in the "Mémoires d'Arcueil," vol. iii.; and in this memoir, after investigating several properties of the chemical rays, he relates the following experiment:—"I received the chemical rays directed into the plane of the meridian, on an unsilvered glass, under an incidence of 35° 6′. The rays reflected by the first glass were received upon a second under the same incidence. I found that when this was turned towards the south, the muriate of silver exposed to the invisible rays, which it reflected, was darkened in less than half an hour; whereas, when it was turned towards the west, the muriate of silver exposed in the place where the rays ought to have been reflected, was not darkened, although it was left exposed for two hours." From this experiment he deduces that the chemical rays can be polarized like white light, when they are reflected by surfaces of glass under a certain angle, and that this angle appears to be very nearly the same for the two kinds of rays. "It is," he says, "consequently to be presumed that the chemical rays can undergo double refraction in traversing certain diaphanous bodies, and, lastly, we may say that they enjoy the same physical properties as light in general."

An experiment similar to M. Bérard's will be found detailed in the following paper, although I was not aware of there being any such on record, till informed of it by the kindness

* Communicated by the Author; having been read before the Royal Society of Edinburgh, December 21, 1840: a communication on the same subject had been previously made to the Literary and Philosophical Society of Liverpool, November 2, 1840.

of Professor Forbes, after my paper had been read. In a communication which I have recently received from him, he also says, "In spring 1839 I tried the experiment of letting the picture formed by polarized light passing through calc spar fall upon sensitive paper, then newly discovered:—whether from the fault of the paper, I know not, but on my first trial I obtained no kind of effect, and my attention being occupied with other matters, I never repeated it: but at Birmingham, in August 1839, being requested to give some account of the Daguerreotype, which I had seen in Paris, I mentioned the experiment, and pointed out its valuable application to fix with unerring accuracy phænomena of diffraction and polarization, which different eyes have seen differently, and which, regarded as the test of theories, would thus be preserved with unimpeachable fidelity for examination at leisure by every eye."

With these few preliminary remarks I shall proceed to the paper itself.

Liverpool, 22d March, 1841.

In the course of last summer it occurred to me that the invisible chemical rays of light might be subject to the laws of polarization, and early in the month of July I instituted a series of experiments to determine the point. In all investigations of this nature it is of importance that the solar light should continue for a certain time of nearly the same intensity; but during the summer the sky was so frequently overcast that a very few days only could be devoted to the subject, and for a considerable part of the autumn the sun's altitude has been too low. For these reasons I have been unable to pursue the investigation so far as I could have wished, but I have nevertheless obtained a sufficient number of results to establish the principal facts, and I have thought it better to bring these forward at the present time than to allow the subject to lie over for the several months which must intervene before I can again resume it.

I have succeeded in polarizing the chemical rays:—1st, as they proceed directly from the sun; 2nd, as they exist at the extreme violet end of the spectrum; 3rd, as they fall from the sky; and by three different processes,—double refraction, reflexion, and repeated single refraction. I shall describe in succession these processes, with the apparatus used.

1. *Polarization of the Chemical Rays by double Refraction.*

The first important fact in regard to the chemical rays is, that they are susceptible of double refraction, in the same manner as the luminous rays are. To prove this, a prism of cal-

careous spar, one and three quarters of an inch in length and one inch in the side, and polished at each end, was employed. It was inclosed in a case having an aperture at one extremity, the other extremity being open. The extreme violet rays of the solar spectrum were allowed to pass through the aperture and to fall on a piece of photogenic paper. Two very faint images were formed, and in a minute or two these produced corresponding dark impressions on the paper. A similar result was also obtained when the direct sun's rays were employed. By this apparatus two impressions of equal intensity, each half an inch long and one-eighth of an inch broad, were obtained; but on extending the experiments I soon found that a polarized beam of greater size than it could give was necessary; I therefore substituted a plate of Iceland spar an inch square and $\frac{3}{16}$ths of an inch thick, and in order to increase the divergence of the rays, one of the planes was ground to an angle of 63° with the obtuse edge, and both planes were then polished. A plate of Iceland spar thus prepared, has the property of separating the two rays so much, that when inserted into an aperture admitting a sun-beam into a darkened room, it gives two images of polarized light, each one inch in diameter and about an inch apart, on a screen placed at the distance of eight feet from the aperture.

These images, when received on sensitive paper, both produced considerable effect; but the extraordinary more than the ordinary, and it was therefore chosen for the purpose of experiment.

An analyzing apparatus, consisting of six thin plates of mica, was placed obliquely in the course of the polarized ray, so as to form with its axis an angle of about 25°. The instrument was turned round until the plane of the mica plates coincided with the plane of polarization of the ray. When this was done the light was almost extinguished, and was allowed to fall on a piece of photogenic paper. After the lapse of five minutes no effect whatever was produced on the paper. The mica plates were then turned round 90°, until their plane was at right angles with the plane of polarization. The light was greatly increased in intensity, and in one minute the paper was tinged, in three minutes a good deal so, and in five minutes it was pretty dark. This experiment proves that the plane of polarization of the chemical rays is coincident with that of the luminous rays of the sun's light.

Instead of the mica plates employed in the last experiment, I next used the long prism of Iceland spar already mentioned. The polarized beam was transmitted along it, and the prism turned on its axis until one of the rays was extinguished: a

piece of sensitive paper received a dark image from the unextinguished ray; but the extinguished ray produced no effect whatever.

A film of mica was then placed in the course of the polarized beam before it passed through the prism, and the extinguished ray immediately reappeared: the two rays were allowed to fall on sensitive paper, and both produced tints of equal intensity.

This experiment was repeated with a film of selenite instead of the mica: one of the rays was coloured of a yellowish, the other of a purple tint: on being received on photogenic paper both the images gave dark impressions, but the purple image produced more effect than the other.

The experiment was again repeated with a film of selenite, which gave a pink colour to one ray and a green colour to the other; and in this instance both images gave tints of equal depth to the paper. These experiments go to prove that the chemical rays, when polarized, are acted upon by thin crystallized plates, in a manner similar to that in which the luminous rays are influenced.

I was next desirous of ascertaining whether any phænomena resembling the coloured rings seen round the axes of crystals in polarized light were presented by the chemical rays when polarized, and for this purpose I employed an apparatus consisting of a tube two inches long and three-fourths of an inch in diameter: at one extremity of it was placed a double convex lens, having a focus of one and a quarter inch: within the tube, and at the distance of half an inch from the lens, was placed a section of a calcareous spar rhomb, such as is used for showing the coloured rings. At the other extremity of the tube was placed an oblique analyzing bundle of three mica plates, or one of Nicol's improved prisms, and the apparatus was so disposed that the polarized sunbeam was allowed to fall on the lens, and thence through the tube upon a screen placed close to it. An image of the coloured rings and black cross was thus obtained, and by turning the tube 90° upon its axis, the rings with the white cross appeared; while in this position a piece of photogenic paper was used to receive the image, and a reversed impression of the rings and cross was obtained; to wit, the place where the white cross had been was dark, the centre light, with a complete black ring round it, and segments of other rings exterior to it. The tube was next turned 90° upon its axis, so as to show an image of the rings with the black cross; sensitive paper was again employed, and another reversed impression obtained; to wit, the position of the black cross was white, the centre and inter-

spaces dark, with segments of two or three darker circles on them.

I have also used a section of rock crystal for the purpose of obtaining impressions of its rings, and to determine whether phænomena were presented similar to those of circular polarization. The result, however, owing to the unsettled state of the weather, was not so satisfactory as I could have desired, and I have therefore left this part of the subject for a more favourable opportunity.

The next step in the investigation was to determine whether similar phænomena were presented at the violet extremity of the spectrum. For this purpose I employed a glass prism to decompose the polarized sunbeam used in the preceding experiments. A polarized spectrum was thus formed, at the extreme violet end of which most of the experiments were repeated. The extreme violet ray was allowed to pass through the prism of calcareous spar, and received on photogenic paper; one image of violet light, extremely feeble, was all that was visible. The action of the chemical ray was, however, intense, for in a minute or two a deep dark spot marked the position of the *unextinguished* ray, while no effect whatever was produced by the *extinguished* ray. A film of mica was now introduced into the course of the violet ray; two faint luminous images appeared, and two dark impressions were obtained. These experiments were again repeated beyond the extreme violet ray. That part of the spectrum which traversed the prism of calcareous spar gave no luminous image, but the result was the same; to wit, the unextinguished chemical ray gave a dark impression on sensitive paper, and the extinguished ray none; and when a film of mica was used, two dark impressions of both rays were obtained.

The experiments with the rings were also tried; but although the impressions were visible, they were by no means so distinct as those obtained from the direct sun-light, a circumstance which is partly to be attributed to the great difficulty of keeping the axis of the apparatus employed in the axis of the polarized ray. The sun's motion has to be compensated by the movement of the hand; and these experiments are on this account of difficult performance, unless a heliostate, or some similar contrivance, be used to keep the sunbeam precisely in the same direction during the required time.

2. *Polarization of the Chemical Rays by Reflexion.*

I have now stated the principal results at which I have arrived in the polarization of the chemical rays by double refrac-

tion, and shall next proceed to detail experiments which go to prove that these rays can be polarized by reflexion. The apparatus made use of consists of a mirror composed of nine plates of parallel glass, by which a beam of polarized light can be thrown upon an analyzing plate of thick flint glass, so mounted that its angle of position can be changed, and its plane of reflexion made to revolve round the polarized beam. The image of the sun, after having been reflected from the mirror at the polarizing angle, was thrown upon the analyzing plate, the plane of which had previously been turned at right angles to the plane of primitive polarization. From this plate the ray was received upon a piece of photogenic paper, and in three minutes a very faint impression was obtained. The apparatus being still in the same position, another piece of paper, cut from the same sheet, was substituted for that used in the last experiment, and the ray was depolarized by interposing a plate of mica between the mirrors; in three minutes the paper received a dark impression, thus affording another illustration of the effect of crystallized plates on the polarized chemical rays.

Another piece of paper, also cut from the same sheet, was made use of, but the analyzing plate was turned 90° upon its axis before the ray reflected from it was allowed to fall on the paper, and in three minutes an impression was obtained equal in intensity to that produced in the last experiment.

These three experiments, which occupied little more than ten minutes in their performance, were executed at a time when the sun's rays were of equal intensity, and the paper used was also of equal sensibility; and they afford an additional proof of the similarity of effect produced by polarizing forces on the chemical and luminous rays.

3. *Polarization of the Chemical Rays by repeated single Refraction.*

To exhibit this phænomenon I prepared two bundles of mica plates, nine in each bundle. These were arranged diagonally in a tube, one half of which could be turned round within the other. The tube was turned so that the planes of both bundles were at right angles, and the sun's rays were transmitted through it so as to fall on sensitive paper. In a few minutes little or no effect was produced, but on turning the planes round so as to coincide, an immediate darkening of the paper took place.

I next employed this method to polarize the chemical emanations proceeding from the sky alone; but as it was necessary in this case to have simultaneous results to obviate the

effects of varying intensity, two sets of experiments were carried on at the same time. A piece of thin window glass was chosen, out of which sixteen plates, one and a half inch long and one inch broad, were cut. These were arranged diagonally in four bundles, and placed in two tubes; two of the bundles having their planes coincident in one tube, and the other two with their planes at right angles in the other tube. The tubes were placed close to each other in a perpendicular position in the open air, so that the light from the sky could pass directly through them upon two pieces of photogenic paper, cut from the same sheet, and placed so as to receive the chemical emanations. In this position they were left for two hours in a tolerably clear day; and although the chemical rays had to pass through the same number of plates in both instances, the impressions received by the paper differed much in intensity, that under the tube containing the two bundles with the planes at right angles being much less affected than the other. The explanation of this phænomenon is that all those chemical emanations which were polarized by the repeated refraction of the first bundle of glass plates did not pass through the second bundle when their planes were at right angles, and consequently produced no effect on the paper; but, on the contrary, they passed readily through the second bundle, when its plane coincided with that of the first, and produced their characteristic darkening effect. The phænomena are in fact similar to those observed with the luminous rays under the same circumstances.

Such, then, are the results at which I have as yet arrived in this interesting branch of physical research, and they appear to prove that the third great division of the solar emanations, like the luminous and calorific, are capable of being acted upon by polarizing forces, and that thus they are all subject to the same beautiful laws.

Before concluding this communication I may state, that the photogenic paper employed in the experiments was prepared in the usual way with chloride of silver, but that it would be more satisfactory to use small Daguerreotype plates, particularly in obtaining impressions of organized or crystalline structures by the solar microscope. To effect this, a pair of short polarizing prisms, made according to Mr. Nicol's improved plan, may be adapted to the microscope, one being placed so as to polarize the sun's light before it falls on the object, and the other to analyze the beam immediately after it has passed the object-glass. A sensitive surface placed so as to receive the image thus formed would take a corresponding impression of the structure.

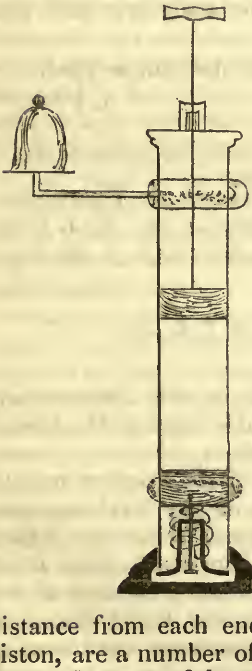
X. *On an Improvement in the Air-Pump. By the* Rev. J. Park.

To the Editors of the Philosophical Magazine and Journal.

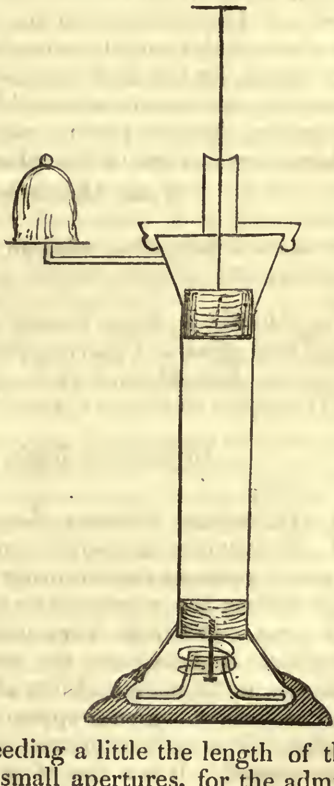
Gentlemen,

YOU will oblige me by inserting in your Journal the following brief description of an improvement in the construction of the air-pump.

A model in my possession works very satisfactorily; but, from want of opportunity, I have not been able to compare its efficiency with other instruments already in use. It consists of a barrel or cylinder of iron truly bored and polished. To this cylinder is accurately fitted a case-hardened iron piston, in the usual manner, the piston-rod working at the upper end through a collar of oiled leathers, perfectly air-tight. At a



Sketch of Model.



Sketch of further alteration not tried in Model.

distance from each end, exceeding a little the length of the piston, are a number of very small apertures, for the admission and egress of the air. At the lower end is *another* accurately fitted metallic plug, the end of which is perfectly flat,

as is the end of the working piston, so that when the latter is forced down, there is no residuum of air left in the barrel at the end of the stroke, between the pistons. There is also a spring to keep the pistons in close connexion till the upper one has returned a little past the openings, by which the air is forced out, a little above which openings the lower plug is retained by a stay coming against the end of the cylinder.

It has been objected that, though the model works well at first, the metallic piston will soon wear, and then the instrument will become inefficient. This may be obviated, I conceive, by making the apertures *very small*, and by using pistons packed or covered with some elastic material, as India-rubber, which would easily pass and repass the openings without tearing or injuring the packing. But in order to dispense with the apertures, and to avoid any objection that might be taken to the use of elastic packing on account of them, it has occurred to me, that the upper and lower ends of the cylinder might be made a little wider than the rest of the barrel, for the inlet and escape of the air. In this case, however, the piston rods would require longer collars to serve as guides, that the pistons might come truly into that part where they fit tight to the cylinder.

I am, Gentlemen, your obedient servant,

J. Park.

Ulverstone, Lancashire, March 13, 1841.

XI. *On* Mr. J. Scott Russell's *Remarks on the Temperature of most effective Condensation (of Steam). By* W. J. Henwood, *Esq., C.E., F.R.S., F.G.S., Secretary of the Royal Geological Society of Cornwall, &c.*

To Richard Taylor, Esq., F.L.S., &c.

Sir,

The article "Steam Navigation" in the *Encyclopædia Britannica* (xx. p. 697), from the able hand of Mr. J. Scott Russell, contains the following remark:—

"It does not appear to be known that a vacuum may be too good. We hear it boasted every day by rival engineers that their engines have the best vacuum. Some boast their vacuum at 27 inches, others at 28, others at 29, some at 30, and at last an engineer appears who boasts a vacuum of $30\frac{1}{2}$ inches. It is to be regretted that time and talent should be thus wasted. It is a fact of great importance, confirmed by experiment and by practice, that a vacuum may be too good, and become a loss instead of a gain."

Similar remarks from the same respectable authority occur in the Proceedings of the British Association at the Glasgow Meeting (p. 186). I fully concur in the statement that a vacuum may be made so good as to be a drawback on the performance of the engine in which it is obtained, but I beg respectfully to represent that this, instead of being now a novelty, has been long known and acted on.

Mr. Farey (Treatise on the Steam-engine, p. 375) says, " Mr. Watt ascertained that if the temperature of the steam and water contained in the condenser of his engines is reduced to 100 degrees, then as the steam remaining in the exhausted space will only have an elastic force equal to a pressure of not quite one pound on the square inch, it is better to leave this weak steam always in the condenser, and also in the cylinder, to oppose the descent of the piston, than to throw in any more injection water, which would be requisite to cool it more, and render it more rare and feeble."

So long ago as the year 1828, I had remarked that the *duty* of Wilson's engine at Huel Towan, was improved by raising the temperature of the hot well within certain limits: and I then calculated, for intervals of five degrees extending from 80° to 100°, the resistance which the residual vapour would oppose to the descent of the piston, the accelerating influence it would exercise on the ascent of the air-pump bucket, and also the greater or less atmospheric action to which that bucket was exposed by the period of discharging the hot water being lengthened or shortened.

The results of these computations appeared in the Edinburgh Journal of Science for 1829 (x. p. 41, O. S.), and I beg permission to quote the remarks by which they were accompanied.

" The quantity of water (to be discharged by the air-pump) should be as small as possible, not so much on account of its weight, as of the greater period during which the piston of the air-pump will be exposed to the atmospheric pressure. On the other hand, the smaller the quantity of water injected, the higher will be the temperature of the hot-well, and consequently the less perfect the vacuum. It is obvious that the smaller the quantity obtained by adding the difference between the impeding influence of the steam remaining in the hot-well on the piston, and its accelerating action on the air-pump, to the whole resistance experienced by the latter during its exposure to the atmosphere, the better will be the operation of the machine."

I have also mentioned the same subject in your valuable pages (Third Series, xiv. p. 491), and in the Transactions of

the Institution of Civil Engineers (ii. p. 60). I am very glad that this important topic has engaged the attention of so able an inquirer as Mr. Russell, but I believe the foregoing extracts will show that the question has not remained unnoticed until now.

I have the honour to remain, Sir,
your faithful humble servant,
W. J. HENWOOD.

4, Clarence Street, Penzance,
May 6, 1841.

XII. *On Cavendish's Experiment.* *By* L. F. MENABREA*.

THE paper before us is a fresh mathematical investigation of the formulæ necessary in the application of Cavendish's experiment for determining the mean density of the earth; and the author seems to have been in some degree incited to the task by hearing of the intended repetition of the experiment in England. It is unfortunate, however, that as Mr. Baily's apparatus is not altogether the same in its details as that of Cavendish, the results of this paper are not wholly applicable to the new observations.

Mr. Menabrea sets out with the well-known equation

$$\frac{d^2\theta}{dt^2} = \int r^2\, dm = \int r\, \phi\, dm,$$

where dm is an element of the system which is moved by the attraction of the large balls, and r is the distance of that element from the axis of the torsion wire. The integral $\int r\, \phi\, dm$ arises from attraction-forces and torsion-forces. The former are,—1st, the attraction of the large spheres upon the small ones; 2nd, the attraction of the large spheres upon the rod which connects the small ones; 3rd, that of the beam (which supports the large balls) upon the small balls; (in Mr. Baily's apparatus the large balls are not suspended from a beam, but are supported on a plank;) 4th, the attraction of the box (which contains the small balls) upon these balls. These are all investigated in the paper before us.

The next step is to calculate the Moment of inertia of the small balls, the connecting rod, and the verniers (in Mr. Baily's apparatus there are no verniers attached to the torsion pendulum). The equation is then integrated, and expressions connecting the earth's density with the data of observation are found.

Mr. Menabrea next proceeds to examine the effect of the resistance of the air upon the time of vibration. Assuming a

* This article is a notice of a paper in the Turin Memoirs (Second Series, vol. ii. p. 305).

formula for that resistance, composed of two terms varying as the velocity, and as the square of the velocity, he finds that the isochronism of the torsion pendulum is not sensibly affected for small vibrations. But he does not (as might be supposed) attempt to assign the small constants which enter the law of resistance, nor to make the resistance itself affect the final result. He examines the effect of the resistance of the air upon the results of Cavendish's method of finding the point of repose of the torsion pendulum, and shows that it is insensible when this point of repose remains in the same place.

The author then examines the manner in which the mean density, obtained from the experiment, combined with the hypothesis that the earth is composed of spheroidal layers of variable density, is to be applied to determine the mass of the earth.

As to the question of mathematics, this paper may be useful in pointing out to future investigators a complete mode of taking into account everything that can affect the result of Cavendish's experiment. But the analysis goes somewhat beyond the practical necessities of the question; a circumstance which is almost inseparable from any attempt to investigate the conditions of a problem which contains so many unknown disturbing forces as the one of which Mr. Menabrea has treated.

XIII. *Reply to* Mr. Airy's *additional Remarks on* Professor Challis's *Investigation of the Resistance of the Air to an Oscillating Sphere. By the Rev.* James Challis, *M.A., Plumian Professor of Astronomy in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE communication you did me the favour of inserting in your Number for June, was written under the idea that some further explanation was required from me of my reasons for differing in the solution of a hydro-dynamical problem from so high an authority as Poisson, and before I was aware that Mr. Airy intended offering any additional remarks on the subject. That communication is consequently not a direct answer to the arguments contained in Mr. Airy's letter in the May Number, and I therefore ask permission to add something in reply. I shall endeavour to be as brief as possible, the discussion having, perhaps, already occupied too much of your valuable space.

Mr. Airy's reasoning consists of two parts. The object in the first is to show, that the motion of a fluid, when directed to or from a centre, must be the same at the same time at all equal distances from the centre. In the other it is argued that motion resulting from two components, one in the direction of a straight line from a centre, the other in a direction perpendicular to this, is inconsistent with the hydro-dynamical equations.

The mathematical reasoning under the first head is the same as if the problem to be solved had been thus enunciated:—To determine the motion of a fluid, assuming the whole velocity to be directed to or from a *fixed* point: in other words, the motion of a given particle is assumed to be *rectilinear*. It will be readily admitted that in this instance the motion is always the same at the same distance from the centre, because if the motions of two contiguous elements at the same distance were at any instant different, their densities would be different, and their motions would cease to be rectilinear. In the case of motion under discussion, viz. that of fluid put in movement by an oscillating sphere, the motion of a given particle of the fluid is obviously not rectilinear, and it is not therefore necessary for me to show why a solution which I propose for this case is not included in the more restricted one of rectilinear motion.

My concern is rather with the second part of the argument. The reasoning here is not brought to a conclusion. By following out the investigation I find that not only the three equations Mr. Airy adduces, but three others also, which have equal claims for consideration, are exactly verified by the kind of motion which Mr. Airy considers to be impossible. I proceed to give the mathematical reasoning.

The letter P being for shortness' sake substituted for the Napierian logarithm of ϱ, the four hydro-dynamical equations, sufficiently approximate for the proposed instance of motion, become

$$\frac{d\,\mathrm{P}}{d\,x} + k \cdot \frac{d\,u}{d\,t} = 0 \quad \ldots\ldots\ldots \quad \text{(a.)}$$

$$\frac{d\,\mathrm{P}}{d\,y} + k \cdot \frac{d\,v}{d\,t} = 0 \quad \ldots\ldots\ldots \quad \text{(b.)}$$

$$\frac{d\,\mathrm{P}}{d\,z} + k \cdot \frac{d\,w}{d\,t} = 0 \quad \ldots\ldots\ldots \quad \text{(c.)}$$

$$\frac{d\,\mathrm{P}}{d\,t} + \frac{d\,u}{d\,x} + \frac{d\,v}{d\,y} + \frac{d\,w}{d\,z} = 0 \quad \text{(d.)}$$

The differential coefficients are all partial: and in any case of fluid motion to which these equations apply, the following equations must be verified:—

$$\frac{d^2 P}{dy\,dx} = \frac{d^2 P}{dx\,dy} \dots (1.) \qquad \frac{d^2 P}{dt\,dx} = \frac{d^2 P}{dx\,dt} \dots (4.)$$

$$\frac{d^2 P}{dz\,dx} = \frac{d^2 P}{dx\,dz} \dots (2.) \qquad \frac{d^2 P}{dt\,dy} = \frac{d^2 P}{dy\,dt} \dots (5.)$$

$$\frac{d^2 P}{dz\,dy} = \frac{d^2 P}{dy\,dz} \dots (3.) \qquad \frac{d^2 P}{dt\,dz} = \frac{d^2 P}{dz\,dt} \dots (6.)$$

The three equations (1.), (3.), and (6.), are the same as Mr. Airy's. Now it appears from equations (a.), (b.), (c.), that the equations (1.), (2.), (3.) are at once verified, if

$$\frac{du}{dy} = \frac{dv}{dx}; \quad \frac{du}{dz} = \frac{dw}{dx}; \quad \frac{dv}{dz} = \frac{dw}{dy};$$

that is, if $u\,dx + v\,dy + w\,dz$ be an exact differential of a function of x, y, z, which may also contain t. Let ϕ be this function: so that

$$u = \frac{d\phi}{dx}; \quad v = \frac{d\phi}{dy}; \quad w = \frac{d\phi}{dz}.$$

Then, as Poisson has shown, (*Traité de Mécanique*, tom. ii. p. 687, 2e édition) it follows, to the same degree of approximation, that $P + k \cdot \frac{d\phi}{dt} = 0$. Hence

$$\frac{d^2 P}{dx\,dt} = -k \cdot \frac{d^3 \phi}{dx\,dt^2} = -k \cdot \frac{d^2 u}{dt^2} = \frac{d^2 P}{dt\,dx},$$

by equation (a.).

Thus equation (4.) is verified: and so for equations (5.) and (6.). It has been shown, therefore, that for the verification of the six equations, it is necessary and it is sufficient that $u\,dx + v\,dy + w\,dz$ be an exact differential of a function of x, y, z, and t, with respect to the three first of these variables. On the same condition the equation (d.) becomes ($k\,a^2$ being $= 1$),

$$\frac{d^2\phi}{dt^2} = a^2 \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}\right) \dots\dots (A.)$$

Hence if this equation be made applicable to a proposed instance of motion, and a value of ϕ be obtained from it by integration, it follows from the preceding general reasoning, that the same value of ϕ will satisfy the six equations above, the only condition necessary for their verification being involved in that equation. This will more clearly appear by taking a particular instance; and as Mr. Airy's argument is not more opposed to my solution of the problem of resist-

ance to an oscillating sphere than to Poisson's, I select the latter for an example, as affording by the verification of the equations the most convincing proof of the possibility of the motion under discussion.

Poisson first transforms the equation (A.) into one of which the coordinates are polar, the centre of the sphere being pole, and takes into account the motion of the sphere. (See Additions to the *Connaissance des Tems* for 1834, p. 40. The equation under the form required for my present purpose is given in my communication to the June Number.) As the coordinates of the centre of the sphere do not appear in the transformed equation, the sphere may be regarded as stationary. The integral obtained by Poisson in art. (8.) of the memoir above cited, is,

$$\phi = \left\{\frac{f(r-at)}{r^2} - \frac{f'(r-at)}{r}\right\} \cos\theta,$$

r being the distance of a point in the fluid from the centre of the sphere, and θ the angle which r makes with the line in which the centre moves. Hence, putting R for the quantity in brackets,

$$\frac{d\phi}{dr} = \frac{d\mathrm{R}}{dr}\cos\theta; \text{ and } \frac{d\phi}{r\,d\theta} = -\frac{\mathrm{R}}{r}\sin\theta.$$

These are respectively the components of the velocity in the direction of r and in a direction perpendicular to r. If the centre of the sphere be the origin of rectangular coordinates, and the axis of z be taken in the direction of the sphere's motion, it may readily be shown that

$$u = \left(\frac{d\mathrm{R}}{r^2\,dr} - \frac{\mathrm{R}}{r^3}\right) z\,x$$

$$v = \left(\frac{d\mathrm{R}}{r^2\,dr} - \frac{\mathrm{R}}{r^3}\right) z\,y$$

$$w = \left(\frac{d\mathrm{R}}{r^2\,dr} - \frac{\mathrm{R}}{r^3}\right) z^2 + \frac{\mathrm{R}}{r}$$

$$\mathrm{P} = -k\,.\frac{d\phi}{dt} = -k\,.\frac{d\mathrm{R}}{dt}.\frac{z}{r}.$$

It is now an easy matter to show that the preceding values of u, v, w, and P, satisfy the equations (1.), (2.), (3.), (4.), (5.), (6.). For example, from the values of P and w,

$$\frac{d^2\mathrm{P}}{dz\,dt} = -k\left\{\frac{d^3\mathrm{R}}{dr\,dt^2}.\frac{z^2}{r^2} - \frac{d^2\mathrm{R}}{dt^2}.\frac{z^2}{r^3} + \frac{d^2\mathrm{R}}{dt^2}.\frac{1}{r}\right\}$$

$$= -k\,.\frac{d^2 w}{dt^2}, \text{ and } \frac{d^2\mathrm{P}}{dt\,dz} = -k\,.\frac{d^2 w}{dt^2}, \text{ by equation (c.).}$$

Again, putting the letter Q for $\frac{d\,\mathrm{R}}{r^2\,d\,r} - \frac{\mathrm{R}}{r^3}$, we have

$$w = \mathrm{Q}\,z^2 + \frac{\mathrm{R}}{r}; \quad \frac{d\,w}{d\,x} = \frac{d\,\mathrm{Q}}{d\,r} \cdot \frac{z^2\,x}{r} + \frac{d\,\mathrm{R}}{d\,r} \cdot \frac{x}{r^3} - \frac{\mathrm{R}\,x}{r^3}$$

$$= \frac{d\,\mathrm{Q}}{d\,r} \cdot \frac{z^2\,x}{r} + \mathrm{Q}\,x$$

and $u = \mathrm{Q}\,z\,x; \quad \frac{d\,u}{d\,z} = \frac{d\,\mathrm{Q}}{d\,r} \cdot \frac{z^2\,x}{r} + \mathrm{Q}\,x.$

Hence $\frac{d^2\,\mathrm{P}}{d\,x\,d\,z} = -k \cdot \frac{d^2\,w}{d\,x\,d\,t} = -k \cdot \frac{d^2\,u}{d\,z\,d\,t} = \frac{d^2\,\mathrm{P}}{d\,z\,d\,x}.$

It is unnecessary to go through the other cases.

This verification, it will be seen, entirely depends on the fact that $u\,d\,x + v\,d\,y + w\,d\,z$ is an exact differential, and is true only in the sense in which this analytical condition is true. It is at this point that the method I have adopted diverges from that of Poisson. From the foregoing mathematical reasoning, it appears that in Poisson's method that condition is not satisfied independently of the particular circumstances of the motion, and of a particular function, cos θ, introduced by considering the nature of the arbitrary disturbance. On the contrary, I argue that as it must be satisfied prior to the consideration of any instance of motion, it must be true independently of all that is arbitrary; and I have shown in the Number of the Philosophical Magazine and Journal for June, that the quantity in question is in this manner an exact differential only when the coordinates are restricted to vary from one point to another in the line of motion. The function ϕ may thus contain implicitly as a factor another function expressing the variation of velocity at a given instant in passing from one point to another in directions perpendicular to the motion, but is not differentiated with respect to the variables of this factor. Accordingly the equation for finding the value of ϕ applicable to the motion caused by a vibrating sphere is

$$\frac{d^2.\,r\,\phi}{d\,t^2} = a^2.\,\frac{d^2\,r\,\phi}{d\,r^2}; \text{ and } \phi = \frac{f\,(r-a\,t)}{r};$$

the factor, cos θ, depending on the mode of disturbance, being included in the arbitrary function. The verification of the six equations by the above expression for ϕ proceeds in the same manner as before, but by simpler operations. As ϕ does not contain explicitly the angular coordinates, the velocity perpendicular to the radius vector is nothing, and the whole

velocity is directed to or from the centre of the sphere. But as the velocity is not the same in all directions from the centre, the motion of a given particle is not rectilinear, but is continually directed to or from the varying positions of the centre. When the motion is wholly in radii from a centre, it is as necessarily different in different directions from a *moving* centre, as it is necessarily the same in different directions from a *fixed* centre.

Although I agree with Mr. Airy that mixed considerations should be avoided in questions of this nature, I cannot forbear entering upon one which appears to furnish a test for deciding between Poisson's results and mine. From what has just been said, it follows that the velocity of a fluid particle at the surface of the sphere *relative* to the surface = velocity of the sphere $\times$ sin θ. Consequently if we impress on the sphere and the fluid the sphere's velocity in the opposite direction, the sphere will be reduced to rest, the fluid will impinge upon it, and the *actual* velocity of a fluid particle along the surface of the sphere will be, the velocity of the fluid $\times$ sin θ. This result, which we can hardly conceive to be in error when the motion is slow, is at variance with Poisson's conclusions.

I have now, I think, sufficiently shown that the results I long since obtained respecting the resistance of the air to an oscillating sphere are legitimately derived from a general hydro-dynamical principle. The accordance of these results with experiment is not of itself a proof of the truth of the principle, and I do not therefore see that any advantage can be gained by further consideration of this particular problem. At the same time that I say this, to avoid fruitlessly prolonging the present discussion, I willingly express the satisfaction it has given me, that the Astronomer Royal, in the midst of his many and arduous engagements, should have thought this subject worthy of so much attention; and though I do not see reason for changing my first views, I acknowledge that they have become clearer on several points by the remarks which this discussion has elicited.

I am, Gentlemen, yours, &c.

Cambridge Observatory, June 19, 1841. J. Challis.

XIV. *Researches on Heat.—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat**. *By* James D. Forbes, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*†

Arts. 1—12, *Laminated and Smoked Surfaces.* 13—29, *Rough Surfaces.* 30—34, *Metallic and other Gratings.* 35—53, *Powdered Surfaces.* 54—65, *Conclusions.*

1. On the 2nd September 1839, M. Arago communicated to the Academy of Sciences of Paris a letter by M. Melloni, containing some very interesting experiments on the transmission of Radiant Heat. M. Melloni finds that rock-salt (which is well known to transmit rays of heat from all sources yet tried with equal facility) acquires, by being

† From the Transactions of the Royal Society of Edinburgh, vol. xv. Part 1.

* The substance of the present paper was communicated to the Royal Society of Edinburgh on the 16th December 1839, in the words of the memorandum which forms part of this Note. The memorandum itself was read, with some verbal explanation and citation of additional facts, on the 6th of January. Every experiment to which reference is made in the present paper, was performed between the 12th November 1839 and the 4th March 1840. Since that time, I have not made a single experiment on the subject. Occupation of other kinds has prevented me from digesting, until now, the results of these experiments, and from stating the grounds of the conclusions which I formerly announced. The present paper, as it stands, having been submitted to the Council on the 15th May 1840, is printed by their authority. The following is the memorandum just referred to, reprinted from the Proceedings of the Royal Society of Edinburgh:—

"On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat. By Professor Forbes.—On the 2nd of September 1839, M. Arago communicated to the Academy of Sciences a letter by M. Melloni, containing some very interesting experiments on the transmission of Radiant Heat. M. Melloni finds, that rock-salt (which is well known to transmit rays from every source with equal facility) acquires, by being *smoked*, the power of transmitting most easily heat of low temperature, or that kind of heat stopped in greatest proportion by glass, alum, and (according to M. Melloni) every other substance. The experiments contained in the Third Series of my Researches on Heat, show that this is equivalent to saying, that substances in general allow only the more refrangible rays to pass; and as M. Melloni had been led by his previous experiments to the same conclusion, his statement amounts to this, that, whilst rock-salt presents the analogy of white glass, by transmitting all rays in equal proportions, every substance hitherto examined acts on the calorific rays as violet or blue glass does on light, absorbing the rays of least refrangibility, and transmitting only the others.

"M. Melloni believes, that the first exception to this rule, or the first analogue of red glass, is rock-salt previously smoked. I desire, however, first to call attention to the fact, that, in a paper published in May 1838 (Researches on Heat, Third Series), I described a substance having similar properties, namely, mica split by heat to extreme thinness, such as I employ

smoked, the power of transmitting most easily heat of low temperature, or that kind of heat which is stopped in greatest proportion by glass, alum, and (according to M. Melloni) every other substance.

2. In the Third Series of these Researches, § 3, I have at-

in polarizing heat. In the month of March 1838, I had established by reiterated experiments, that the transmission of heat through glass, far from rendering it less easily absorbed by mica in this peculiar state, had a contrary effect, and also that heat of low temperature, wholly unaccompanied by light, was transmitted almost as freely as that from a lamp previously passed through glass.

"It even appears, from experiments I have since made with the same form of mica, that some specimens transmit *scarcely half* as much luminous heat previously passed through glass, as that from a body below visible incandescence.

"Mica itself, not laminated by the action of fire, possesses, as I have shown by contrasted tables in the paper referred to (Art. 23, 24), properties exactly the reverse; hence the effect is due to the peculiar mechanical condition of the body, and not to its elementary composition.

"It, therefore, at once occurred to me, on reading M. Melloni's communication, that the effect of smoking the salt might be merely owing to a mechanical change in the surface affecting the transmission.

"Roughening the surface was the most obvious experiment, and I found, as I anticipated, that heat of low temperature is very much easier transmitted by salt scratched by sand-paper in two directions at right angles, than luminous heat. Thus, a plate of salt which, when well polished, transmits 92 per cent. of heat derived from a lamp, and sifted by a glass plate, and also 92 per cent. of heat wholly unaccompanied by light, transmitted, when roughened, only 17 per cent. of the former and 45 per cent. of the latter.

"A thin plate of mica, when similarly scratched with emery-paper, so as merely to depolish it, transmitted much more nearly the same per-centage of heat from different sources than when *bright;* showing that the loss of polish affects the transmission of the more refrangible rays much more sensibly that that of the others.

"Yet this effect is not attributable to a variation in the ratio of the reflexion of heat of different kinds at the surfaces of the plate. For, in the *first* place, I have proved, and already communicated the fact to the Royal Society (see Proceedings for April 1839), that reflexion takes place at a polished surface, with almost, if not exactly, the same intensity for all kinds of heat; and, *secondly,* I have found, by direct experiment, that, at least for the higher angles of incidence, reflexion is most copious from rough surfaces for heat of low temperature, or the same kind which is most freely transmitted, proving incontestably that the *stifling* action of rough surfaces is the true cause of the inequality.

"That there is a real modification of the heat in passing through a roughened surface, as well as through laminated mica and the smoky film, appears from direct experiments which I have made on the heat sifted by these different media; which, when transmitted by any one of these, is found in a fitter state to pass through each of the others; and this modification is found to be more perceptible as the character of the heat is more removed from that which these media transmit most readily, that is, as the temperature of the source is higher. Thus, heat derived from a lamp, has 36 per cent. transmitted by a certain smoked plate of rock-salt. But if the heat transmitted by the smoked salt has previously been sifted or ana-

tempted to demonstrate, directly and numerically, that the rays of heat which have passed through alum, glass, and indeed every substance which I tried, have a *mean refrangibility* superior to that of the rays before such transmission; and as M. Melloni had been led in a general way by his previous experiments to a similar conclusion, he inferred, and justly, that most diathermanous bodies absorb the less refrangible rays in excess, and therefore are to heat what green, blue, or violet diaphanous media are to light. Rock-salt alone (so far as we know) possesses the property of *indifferent diathermancy*, and is the single analogue of white transparent glass.

3. The generalization of this principle is a matter of much importance, and especially as it carries our knowledge a step higher in the scale of truth, by teaching us to refer to the quality of *refrangibility* certain properties of heat, which before were connected only with certain vague characters of the nature of the source whence it was derived. Amongst other things we find, what was long suspected, but what M. Melloni first conclusively proved, that the presence or absence of light is, to a great extent, immaterial; no doubt a *concomitant*, but

lysed by transmission through another plate of smoked salt, through laminated mica, and through roughened salt, the per-centage is raised from 36 to 41 in the two former cases, and to 40½ in the latter, proving incontestably the specific action of these transmissions in arresting the more refrangible rays.

"I next considered, that as a moderate number of scratches appeared to produce this modification, it might be practicable to obtain the effect by transmitting heat simply through fine wire gauze. I could not obtain it finer than sixty wires to the inch, and in this case, I could obtain no indications of differences in the transmitted ratios of one or other kind of heat. The proportion transmitted to the direct effect, was, in every case, almost exactly that of the area of the interstices of the gauze to its entire surface.

"When fine gratings (used for Fraunhofer's interference fringes) made of cotton-thread were used, even in this case no difference was perceived; here, however, the thread, having probably a certain degree of permeability, might mask the effect.

"When fine powders were strewed between salt plates, leaving minute interstices, the easier transmission of heat of low temperature was again apparent.

"Having procured delicate lines to be drawn with a diamond point on a polished salt surface, first dividing it into squares 1–100dth inch in the side, then into parallel stripes 1–200dth inch apart, and finally into squares of the latter dimension, in every case the effect resembled that of random scratches, and was more apparent as the surface was more furrowed.

"I have finally to observe, that the mere process of natural tarnishing by the exposure of salt to the air, produces a similar effect.

"These facts evidently point to phænomena in heat, resembling diffraction and periodic colours in light. I cannot doubt that the simple transmission through fine metallic gratings would produce effects similar to those of the striated surfaces of rock-salt.—*December* 16, 1839."

not an *indispensable* circumstance. Again, certain relations had been established at an early period in the history of the science of heat, between the *colour* of a surface and the quantity of heat which it absorbed, and this relation for any two surfaces compared (as black and white, of similar textures), was first clearly shown by Sir John Leslie, to depend upon the luminosity of the source of heat, to which conceiving it proportional, that philosopher based upon it the principle of his Photometer *. Professor Powell, of Oxford, conceived and executed an ingenious experiment, by which it is demonstrated that the interposition of a screen of glass, though it stops but little light, alters most materially the influence of colour on the transmitted heat, thus annihilating at once the principle of photometric measurement adopted by Leslie, except in a very limited class of cases†. M. Melloni has fully confirmed the experiments of Professor Powell‡, which therefore may be considered as establishing this conclusion, that the quality of *blackness* or *whiteness* of a surface affects its power of absorbing heat (*not* in proportion to the luminosity of that heat, as was formerly supposed, but) in proportion to its refrangibility.

4. It is both convenient and correct, therefore, to consider the refrangibility of heat as the cause of most of its distinctions of *kind* and *degree* of modification in our experiments, instead of making vague reference to the temperature of the source whence it is derived. Heat derived from the following scale of temperatures corresponds to heat of progressively elevated refrangibility; as, 1. Heat from ice has a less refrangibility than that from, 2. the hand, which again is below, 3. that from boiling-water: then comes, 4. that from a vessel of mercury under its boiling temperature, 5. a piece of smoked metal, heated by an alcohol lamp behind, but itself quite invisible in the dark, 6. incandescent platinum (a coil of wire in an alcohol flame), 7. an oil lamp (Locatelli's). Such is the scale of heat which has often been referred to in M. Melloni's researches and my own; but though our apprehension of the temperature of the source ceases to be so clear above this limit, and the colour and brightness of the light which accompanies the heat no longer varies distinguishably, the scale may

* Essay on Heat, 1804. † Phil. Trans. 1825, p. 187.

‡ *Ann. de Chimie, Avril* 1834. M. Melloni finds, for instance, that the rays from an oil-lamp falling on black and white surfaces, affects their temperature in the proportion of 1000 : 805. And the same proportion holds if they be transmitted through a plate of rock-salt; but if a plate of alum be used, though equally transparent for light with the salt, the proportion is now 1000 : 429.

be carried upwards indefinitely by interposing screens of different materials, which either may be proved *directly* (as I have done in the Third Series of these researches) to increase the refrangibility, or we may take Professor Powell's, or any similar test, which our experiments lead us to conclude to be co-ordinate with the fact of refrangibility. Such a prolongation of the scale of heat-sources would be,

8. Oil-lamp heat transmitted by Common mica.
9. ——————————— Glass (Argand lamp).
10. ——————————— Citric acid.
11. ——————————— Alum.
12. ——————————— Ice.

A clear appreciation of the scale of refrangibility as the important test for the qualities of heat cannot be too clearly apprehended and admitted. Heat from *any* source, if it admit of transmission at all through glass, alum, or water, will ultimately have the character of glass-heat, alum-heat, or water-heat, just as light from the sun, or from a candle, becomes red, blue, or green, by transmission through glasses of these colours.

5. Now, when M. Melloni had shown (and this experiment I believe was original to him), that substances which stop every ray of even intense light (as opake glass and some kinds of dark mica), yet transmit a sensible quantity of heat, it was not unnatural to inquire whether the *invisible heat* thus obtained from a *luminous* source, might not possess the qualities of heat from a dark source, in other words, whether bodies, like black glass and mica, instead of stopping the less refrangible rays like glass, alum, &c., would not suffer these to escape, and absorb the most refrangible rays, acting upon heat as a body does upon light, which stops the yellow, blue, and violet rays, that is, as red glass does.

6. Experiment partly fulfils this expectation, and partly not. The careful and complete series of experiments made by M. Melloni upon the qualities of the invisible heat thus obtained*, shows, that although it resembles low-temperature-heat, in so far as it is very feebly transmitted by alum or citric acid, yet low-temperature-heat (that from boiling water for instance) is but very faintly transmitted through the black glass or mica, which ought not to be the case if these bodies acted like a sieve, which arrested the more refrangible rays, and suffered the others to escape.

7. The direct test, however, of examining the refrangibility of the heat rays issuing from opake screens yet remained; and

* *Annales de Chimie, Avril* 1834.

in applying this, I proved that opake glass and mica act as clear glass and mica do in *elevating the mean refrangibility of the transmitted heat.* Hence I concluded that the effect of such media upon heat is to absorb the rays of greatest and least refrangibility, in short, to act as homogeneous yellow glass would do upon light, the *mean* refrangibility being on the whole, however, increased by transmission. I also pointed out that heat from luminous sources is probably far more compound in its nature than dark heat; that the darkness of heat is no test of its refrangibility; and that even the most refrangible rays may contain heat separable from the light which accompanies it*.

8. In all this, then, there appears nothing exactly equivalent to the action of red glass upon light,—no substance which transmits most easily heat of low refrangibility and temperature, and which separates heat of that description from the compound emanation from luminous sources. Reasoning probably upon the conclusions just stated, M. Melloni conceived the happy idea of combining an opake substance, such as smoke, with a solid, which itself should effect no specific change upon the incident heat. He therefore smoked *rock-salt*, and found that it presented a complete analogy to red glass, transmitting most easily heat of low temperature and refrangibility.

9. Whilst I give full credit to M. Melloni for the ingenuity and importance of his experiment, I must be permitted to state, that I conceive that I preceded him by eighteen months in the discovery of a substance possessing similar properties, although I very readily admit, that, having been led to that observation incidentally, I first pursued the remark into consequences which I considered important, after M. Melloni had called particular attention to the experiment with smoked surfaces. On the 27th February, 19th and 20th March 1838 (as appears by my Journal of Experiments), I proved that mica, split into very thin films by the action of heat, such as I employ for polarizing, possesses the property of transmitting in larger proportion several of the less refrangible kinds of heat, and in particular, that it transmits heat from a source perfectly obscure, in almost exactly the same proportion with the highly refrangible heat of a lamp transmitted through glass. I have no hesitation in saying, that no other substance known previously to M. Melloni's experiments with smoked salt, gave any approximation to the following results, which are taken from the Third Series of my Researches, art. 24.

* Researches on Heat, Third Series, art. 73, 81, &c.

Table of the proportion of Heat from different sources transmitted by the Polarizing Mica Plates I and K, contrasted with the transmissions by Mica in its usual state, and with Black Glass.

Source of Heat.	Mica split by Heat, Plates I and K.	Mica ·015 inch thick.	Opake Black Glass*.
Locatelli lamp, with glass...	100	100	100
Locatelli	116	79	70
Incandescent platinum	108	70	...
Brass at 700°.................	96	21	...
Heat at 212°..................	62	11	...

* A contrast experiment made at the same time, March 20, 1838.

10. This singular result of the mechanical condition of the mica did not fail to strike me greatly at the time, and was not published until after careful repetition. It afforded a triumphant reply to an objection against my experiments which I was then combating, that the quantity of heat absorbed by the polarizing plates had modified and even inverted the results, and having satisfied myself of that, I did not pursue the matter further. The moment, however, that I read M. Melloni's communication on Smoked Salt, I perceived the important light which the perfectly analogous case of the split mica might throw upon the phænomenon. It was evident that the results were similar in kind, it was probable that they might be made to approximate in degree. Instead, therefore, of interposing mica piles at the great and disadvantageous obliquities which I had employed (when I wished simply to test their action as polarizing plates), I took a split mica pile (frequently referred to in former parts of these memoirs under the designation H) and placed it *perpendicularly* to the incident rays of heat. I obtained the following results:—

Transmission through Split Mica H, at a Perpendicular Incidence.

Source of Heat.	Per 100 of Incident Rays.	Relative Transmission.
Locatelli, with glass	9·2	100
Locatelli	13·7	150
Dark hot brass	17·3	188
Hot water	16·3*	178

* This observation having been made at a different time from the others, and probably not under exactly the same circumstances, I have stated it in the way least favourable to the views I entertain: the per-centage actually observed was 19.

11. It appears, then, very clearly, that this peculiar condi-

tion of mica induces, in opposition to the natural quality of the substance (9), the same peculiarity which a film of smoke possesses relatively to the incident heat. It is truly for heat what red glass is for light, it transmits most freely rays of lowest refrangibility.

12. Seeing clearly from the first that the change of character in mica was due to the splitting up into an almost infinite number of minute surfaces the natural laminæ of the mineral mica; and attributing the character of redness (so to speak) to the multiplied and irregular reflexions and interferences which must so take place, it occurred to me as very probable, that the effect of smoke was due to the superposition of a prodigious number of minute opake points upon a transparent surface, and *that* not so much from any physical peculiarity of its carbonaceous material, as from the mechanical distribution of opake dust over the diaphragm of rock-salt.

13. This induced me to try the effect of *mechanical alterations of the physical surface* of the salt, expecting to find an effect analogous to that of smoking, and, guided by no other grounds of conjecture than those which I have stated, I roughened with sand-paper both sides of a polished plate of rock-salt, furrowing each surface rectangularly until it was quite dim. I then examined its transmissive power for heat from different sources, and was gratified to find my anticipation realized. The proportion of dark heat transmitted, compared to that from a lamp sifted by glass, was no less than as 3 to 1*.

* I state it as a proof of the conviction which I had of the real character of split mica with respect to heat, that the reasoning stated in the text was founded upon no experiments made subsequently to those of March 1838 already quoted. The very first entry in my journal-book of last autumn contains *simultaneous* experiments, (1.) on smoked salt, to verify M. Melloni's observations: (2.) on split mica, to extend my own of March 1838 to perpendicular incidences: (3). on scratched surfaces, on the assumption that the two former would be realized. As M. Melloni thinks that I had not a clear idea of the properties of split mica, which, indeed, if I understand him, he still doubts, I will quote *verbatim* the passage in my laboratory-book alluded to.—" 1839, Nov. 12. M. Melloni having lately stated (*Comptes Rendus*, 2nd Sept.) that smoked rock-salt is the only substance known which transmits heat of low temperature easier than luminous, this is in the first place contradicted by my experiments of 1838, Mar. 20. &c. on mica split by heat, already published,—and in the next place, I felt [feel] some doubt whether [in his experiments] it was the quality of the *material* or only the *surface* which affects the result. To try this, and to verify previous experiments, I smoked a plate of rock-salt; I *roughened* another with sand-paper, first on one, and then on both surfaces; I had also the split mica plate marked H placed *perpendicularly* to the rays of heat."

[*Here follow the experiments.*]

" It clearly appears, then, that salt simply roughened transmits most dark heat. I presume that the effect of smoking is only superficial, and that roughening stifles luminous heat faster than dark heat."

This is the *first* entry in my book after the publication of M. Melloni's letter in the *Comptes Rendus*, and it is given *entire*.

14. It thus appeared that there are at least three conditions under which a medium can be found capable of transmitting heat of low refrangibility, and that two of these had reference *solely* to mechanical constitution. It was natural to generalize and attempt to include the case of the film of smoke, as well as the striated and the laminated surface, under one category. I have already said that the mechanical distribution of the opake carbonaceous particles offered a plausible analogy, which I proceeded to attempt to carry out.

15. The numbers in art. 10, may be compared with the following:—

Source of Heat.	Transmission per 100 of Incident Rays, by		Relative Transmission by	
	Smoked Salt.	Rough Salt.	Smoked Salt.	Rough Salt.
Locatelli, with glass ...	30	49	100	100
Locatelli	...	62	...	126
Dark hot brass	58	70	192	142
Hot water	67	77	223	157

16. It occurred to me that if the action of the smoke was entirely a *superficial* one, or due to the character of a rough surface applied to the plate of rock-salt, that the effect of two such surfaces upon the transmission of heat would probably differ from that of a single film of smoke, so thick as to produce an equal absorption of heat of any particular degree of refrangibility. For this purpose I smoked three plates of polished rock-salt, so that two marked D and E absorbed *together* as much dark heat (very nearly) as the third plate A did alone.

17. I may take this opportunity of mentioning the way in which I have succeeded in smoking inflammable surfaces without burning them, or crystallized plates, like rock-salt, which crack and fly by the direct application of the flame of a candle. A coarse gas-flame, surrounded by a wide metal tube 10 or 15 inches long, against the side of which the flame partly plays, affords a stream of comparatively cool smoke, which may be applied to any given surface. With these three smoked salt-plates I obtained the following results:—

	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Heat.
	Per cent.	Per cent.	Per cent.
Smoked Salt Plate A	8·3	17·2	32·9
———————— D	26	41	58
———————— E	23·5	36	53·5
———————— (D + E)	7·3	18	32·1

As most of these results are from single experiments, the first and the last line must be considered as almost identical, and certainly do not indicate any material specific difference in the absorbent qualities of one thick and two thin films of smoke, which might be expected if the action were a merely superficial one.

18. From these numbers we deduce another conclusion of some importance. Since a film of smoke transmits most easily heat of low temperature and refrangibility, we may expect that it will modify the quality of any compound beam of heat which it transmits, and that one such transmission will therefore render a second more easy. Now, we find that the plate D transmitted 26 per cent. of heat from the first of the above sources, and that of the 26 rays escaping from D, and falling upon a second smoked film E, E transmitted 7·3, or 28 per cent. of those incident upon it. But by the third line of the table E transmitted 23·5 per cent. only of the direct rays, consequently the capacity for transmission has been increased. In the same way for Locatelli heat we find the percentage for E raised from 36 to 44 by previous transmission through D; and for dark heat from 53·5 to 56.

19. Hence a useful application of smoked surfaces to which I have sometimes had recourse. It is often important to operate with more or less refrangible rays of heat under exactly the same circumstances of parallelism or divergence, and intensity. Having adjusted an oil-lamp with a salt lens, so as to afford a compound beam stronger than required, we may, by interposing a plate of smoked salt, absorb the most refrangible rays, and suffer the others alone to pass, and by then using a glass of proper thickness, the intensity of the heat may be reduced in the very same proportion, but the more refrangible (hottest) rays are alone retained*.

20. Now the results of (17), though not what I anticipated as most probable, do not altogether relieve us from some doubt as to the nature of the action of the film of smoke, although those experiments, as well as others which are to be detailed in this paper, incline me to M. Melloni's opinion, that the

* *Smoked glass* is evidently an excessively opake compound medium, being composed of two parts which absorb opposite ends of the heat spectrum. It is curious to reflect how little the true cause of the opacity of a film of smoke deposited upon glass was understood at the time that it was quoted as a convincing proof of the *immediate* radiation of heat through solid bodies. Far from smoke being the untransparent substance supposed (I use the word loosely in applying it to heat), it transmits a quantity of some kinds of heat really surprising, although the thickness of the smoke be considerable.

smoke acts by its own intimate constitution, and not by its mechanical arrangement. Though I have examined smoky films with a powerful microscope, I have failed in detecting the minutely divided particles of carbonaceous matter of which it must undoubtedly consist. Still the reticulation which fine powder strewed on a surface must form, if it act by the minuteness of the spaces which are left (as in diffraction-experiments on light), must act more intensely when by superposition such reticulations become more minute and complicated. And it may little matter whether the smoky screens are distinct, and deposited on separate plates mechanically placed in succession, or whether they are accumulated by continued smoking on a single surface. I do not state this with a view to maintain my own original opinion, which I am rather disposed to abandon, and to consider a smoked surface, *diathermanous*, as well as *transparent*, in the full meaning of the words; but in extending my experiments to roughened surfaces, I was rather surprised to find that the continued action of furrowing the surface by scratching it with coarse sand-paper, not only diminished the transmission of heat, but increased the *specific* action on rays of different refrangibility, whilst one would rather have imagined that the action being here due to the destruction of polish, and therefore *superficial*, any exaggeration of the roughness would not have increased the relative *diathermancy* to rays of low refrangibility.

21. Conclusive experiments, however, mark an increased sensibility to various kinds of heat by increased roughness. Two plates of salt, marked *a* and *b*, having been scored with sand-paper in rectangular directions on both sides, were placed so as to intercept similarly a parallel beam of heat. The difference of the following numbers is due to the less degree of roughness of *a*.

	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.
	Per cent.	Per cent.	Per cent.
Rough Salt Plate *a*	30	48·5	59
——— *b*	16·6	28·5	45
——— $(a + b)$ ...	7·2	16	27·5
Per-centage of heat received through *a* transmitted by *b* ...	24	33	46·5
Ratio of *a* to *b*	100 : 55	100 : 58·5	100 : 76

Here, then, we find the per-centage of transmission raised in every case by a previous transmission through a rough surface. The increased facility of transmission is greater in proportion as the incident heat was more heterogeneous; dark heat undergoes very little change. It appears also by the last line of the table, that the increased roughness of *b* compared to *a*, had *enhanced* the characteristic effect (analogous to *redness* for light).

22. I have made a great many experiments to satisfy myself that the action of all the three media already specified (14) is precisely analogous, and that they actually insulate similar rays by absorption. The following table is a specimen, showing the increased facility with which rays of heat, from whatever source, are transmitted by smoked rock-salt after previous transmission through the same or other substances.

Table showing the Per-centage of Transmission by the Smoked Rock-Salt Plate E for heat from different sources, and modified by passing through the following Media.

Source of Heat.	Heat transmitted by			
	Nothing.	Split Mica H.	Smoked Salt D.	Rough Salt *a*.
Locatelli, with glass ...	23·5	...	28	29
Locatelli	36	43·5	44	40·4
Dark hot brass	53·5	56	56	55

23. It is very important to consider how this action of rough surfaces may be explained, and whether we have any analogous phænomena in the case of light. Can it be owing to the circumstance that the depolished surface reflecting differently the various kinds of heat, those kinds least copiously reflected persevere, and form the majority of the transmitted rays? To this it may be replied, that the intensity of reflexion at polished surfaces is so insignificant at a perpendicular incidence for either heat or light*, that were the *whole* specularly reflected heat, transmitted in the one case, and absorbed in the other, the difference, instead of amounting to 30 per cent. or more, of the incident heat (21), could not exceed 4 per cent.

24. Arguing from the analogous case of light, I anticipated, on the contrary, that the *reflected* as well as the *transmitted* beam, would be more intense from such a surface, as it is well known that polish becomes more specular for rays of light con-

* See Melloni, *Ann. de Chimie,* Dec. 1835, and my Memorandum on the Intensity of Reflected Heat and Light, Proceedings of the Royal Society of Edinburgh, p. 254.

sisting of longer undulations, the inequalities of the surface first becoming insignificant for red light.

25. In this I was not deceived. My purpose not being to investigate fully the subject of diffuse reflexion, I confined my attention to the establishment of the general fact. Employing an apparatus which I have not yet described, but which bears a great analogy to that figured in the Society's Transactions, vol. xiv. pl. xiii., and described in art. 51 of the Third Series, I observed the intensity of reflexion of heat from different sources at a *single* polished surface of flint-glass, and at a similar surface depolished with emery. I obtained at considerable incidences the following striking results as to the increased susceptibility of heat to be *regularly* reflected at a rough surface, when it is of low temperature or refrangibility.

Ratio of the Intensities of Heat reflected by a Polished and a Rough Surface of Flint-Glass.

Angle of Incidence.	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.
60°	...	100 : 34	100 : 35·4
70	100 : 26·5	100 : 38·3	100 : 43·5

So far then the character of the action of depolished surfaces is consistent. *The stifling effect* (which diminishes both the reflected and refracted ray) *of a rough or laminated surface diminishes with the refrangibility of the incident heat.* That the same thing takes place in the reflexion of light we know; it is probable that it does so in its transmission likewise, though this has not been so distinctly observed. Most impure substances transmit a ruddy gleam, vapour of water does so whenever it is not colourless*, and every practical optician knows, that in a great majority of media the violet end of the spectrum is first absorbed.

[To be continued.]

XV. *Notices respecting New Books.*

On the Theory of the Moon, and on the Perturbations of the Planets. Part IV. [With a "*Note on the Calculation of the Distance of a Comet from the Earth.*"] By J. W. Lubbock, Esq., Treas. R.S., Vice-Chancellor of the University of London, &c.&c. Lond. 1840. 8vo. Pp. xiv., 355–417, and 1–6.

In the former parts of this work (the first of which was noticed in our fourth volume, p. 218) the author endeavoured to explain

* Edinburgh Transactions, vol. xiv. p. 371.

methods of determining the inequalities of the moon, and the periodical inequalities of the planets, which appear to him preferable to any which have yet been proposed. He observes, in the Preface to the part now before us, that the expressions for the variations of the elliptic constants discovered by Lagrange, were not recommended by that illustrious mathematician to be employed for this determination, although Poisson afterwards advised that they should be used; but if his valuable life had been spared, the author observes, " it is possible that his opinions upon this point would have been modified." Mr. (now Sir John W.) Lubbock then proceeds to quote a letter addressed to him by M. de Pontécoulant, in which the methods of solving the difficult problem of the theory of the moon successively employed by Clairaut, d'Alembert, Laplace, Damoiseau and Plana are summarily discussed; the remarks of M. de Pontécoulant being introduced by the observation, " it will be seen that they coincide with the opinions I have expressed in various places during the course of this work respecting the manner in which the perturbations of the moon can be computed with the greatest facility."

" The introduction of auxiliary variables," it is next remarked, " offers a wide scope to the imagination. Recently, M. Hansen has proposed to arrive at the expression for the longitude by altering the time in the elliptic value. It is evident that a celestial body will have, at any instant, the same longitude which it has at some previous or subsequent instant in the elliptic movement; and if this difference of time be calculated, the longitude can be obtained from the elliptic expression by substituting for the true time some other quantity. The radius vector cannot be calculated from the elliptic expression by the same alteration of the time; but M. Hansen gives an expression, by means of which subsidiary terms may be obtained, which, added to the radius vector calculated from the elliptic expression, may give its proper value."

M. Hansen's method being applicable to all mechanical problems, the author proceeds to illustrate it by a simple case, supposing

$$\frac{d^2 x}{d t^2} + x + \alpha x = 0,$$

and considering αx as the disturbing function, and neglected, at first,

$$\frac{d^2 x}{d t^2} + x = 0,$$

of which the solution is

$$x = a \sin (t + b),$$

a and b being constants. If $R = \frac{\alpha x^2}{2}$, $\alpha x = \frac{d R}{d x}$.

When the disturbing function is retained a further approximation may be obtained by one of the three following methods, exemplified by the author:—

1. By substituting the value of x, $a \sin (t + b)$ in αx, and proceeding by the method of indeterminate coefficients.

Or, 2. By finding values of a and b considered as variable, which, substituted in the expression

$$x = a \sin (t + b),$$

will satisfy the differential equation.

Or, 3. By finding a quantity z, such that

$$x = a \sin (t + z + b),$$

so that x is the same function of $t + z$ in the disturbed, as of t in the undisturbed motion. The two latter of these methods are more fully developed.

This case, illustrative of M. Hansen's method, is investigated at large; and the author, stating that in the more complicated question of the determination of the longitude of the planets and their satellites, the reasoning is precisely similar, proceeds to exemplify its application to that problem, in accordance with the expressions previously obtained by Laplace and Pontécoulant.

The increased labour, trouble, and difficulty of the method proposed by M. Hansen, are then pointed out, and the special discussion of that method is closed by the following observations:—

"It is impossible to estimate the policy of adopting M. Hansen's methods sufficiently without considering at the same time the actual state of the lunar theory. M. Plana, after much labour, exhibited results obtained with no less skill than honesty of purpose, and containing few numerical mistakes. His results have been examined by me, and more extensively by M. de Pontécoulant; and as we pursued an independent process, it is probable that few if any errors remain undetected in M. Plana's expressions for the longitude and latitude of the moon. If M. Hansen contemplates the exhibition of the value of z to the same degree of approximation, and not developed according to powers of m, such an expression will be incapable of verification, and useless. If the value of z be developed according to powers of m, no opinion will be possible of its accuracy until it has been verified independently by other mathematicians, for only the terms which are independent of the eccentricities will be found in M. Plana's expression for the longitude."

So far as these observations relate to the great work of Plana on the lunar theory, they may, we think, be instructively placed in apposition, by the student of analysis and of physical astronomy, with the character of M. Plana's labours given by Sir John F. W. Herschel, in his address to the Royal Astronomical Society on the subject of the award of the medal to the Italian astronomer and analyst, which has already been transferred to our pages. (Phil. Mag. Third Series, vol. xviii. p. 153.)

The most advantageous method of calculating the perturbations of the small planets, is stated by Sir John Lubbock to be that described by M. de Pontécoulant (*Théor. Anal.*, vol. iii. p. 505), which he briefly recapitulates, and next proceeds to compare with the method of M. Hansen, pointing out the far greater facility of the operations required in the former; and terminating at once the consideration of the subject and the Preface to the work, with the subjoined reflections:—

"If the form of the present planetary tables be abandoned which give the longitude, and tables be formed which give the value of z, most of the numerical calculations contained in the third volume of the *Méc. Cél.* will be thrown away. It would be better to subject those figures, and particularly the quantities b, to a careful examination.

"Public institutions, established with the view of contributing to the progress of astronomy, would confer more essential services to science if their strength were devoted to purposes of this kind rather than to the inconsiderate multiplication of unreduced or not wanted observations. The determination of the perturbations of the small planets is a matter of urgency. Moreover, the perturbations of the comet which bears the name of Encke, are of too great cosmological importance to be properly entrusted to any single hand, however skilful and conscientious, or indeed even to a single method. But such troublesome computations, to be conducted with a fair chance of success, must be undertaken with the requisite appliances, and seem therefore properly to fall under the superintendence of those astronomers who are placed in public situations. Mutual cooperation upon a uniform system would produce more important results than any incoherent computations founded upon fantastical expressions."

The applicability of the views suggested in the preceding paragraph to the future proceedings of the British Association, the Royal Astronomical Society, and other scientific bodies concerned in the reduction of astronomical observations and the formation of new astronomical tables, will immediately be perceived.

The contents of the work itself (in this fourth part), are distributed in the following order:—

Calculation of the first term in the longitude, appertaining to the arguments 0, 1, 2, 3, 4, 5, 6 and 7, in illustration of M. Hansen's equations.—Calculation of terms depending upon the second approximation, in further illustration of M. Hansen's equations.—Calculation of the first term in the reciprocal of the radius vector and of the longitude of the moon, by means of the variation of the elliptic constants.—On the variation of the arbitrary constants in mechanical problems.—On the variation of the semi-axis major of the moon's orbit.—On the divergence of the numerical coefficients of certain inequalities of longitude in the lunar theory.

In the calculation of the first term in the longitude, appertaining to the arguments 0, 1, 2, 3, 4, 5, 6 and 7, in illustration of M. Hansen's equations, Sir John Lubbock adopts an equation and expressions given by M. Encke, in his paper entitled "*Uber die vom Director Hansen eingeführte Form die Störungen in unserm Sonnen-system vollstandig zu entwickeln.*" He then shows in what manner the same terms may be calculated by his own methods.

The terms depending upon the second approximation are next calculated, in further illustration of M. Hansen's equations; and then the first terms in the reciprocal of the radius vector and of the longitude of the moon, by means of the variation of the elliptic con-

stants. In treating of the latter subject, the author shows, by an elementary example, the kind of operations which would be required in order to determine the periodical inequalities of the moon or planets by means of the variation of the elliptic constants, as proposed by Poisson; exemplifies a modification of that method arising from the introduction of a certain constant quantity; and concludes the section by comparing with its previous contents the calculation of the same terms according to his own methods.

The next subject is the variation of the arbitrary constants in mechanical problems, commencing with a view of the theory of that variation, substantially identical with the author's paper on the same subject inserted in our eleventh volume; and followed by the extension of the methods employed in that paper to a more general case, as given in our twelfth volume.

This is followed, under the head "*On the Variation of the Semi-axis Major of the Moon's Orbit,*" by the author's demonstration of the theorem that the expression for that Variation contains no argument of long period, accompanied by a multiple of m less than m^4. As this had previously appeared at length in our pages (Third Series, vol. xvii. p. 338), we need not notice it further at present.

The concluding section, on the divergence of the numerical coefficients of certain inequalities of longitude in the lunar theory, is thus introduced:—

"The divergence of the numerical coefficients in the lunar theory, made manifest by M. Plana's development of the expressions according to the powers of m, presents a difficulty in a complete numerical solution of the problem, that is, a solution intended to embrace all quantities which are sensible in practically ascertaining the moon's place with the accuracy required for comparison with the best observations. But the following questions naturally occur: Is there any method of approximation which will serve to select the more considerable terms, rejecting others? Is the divergence due chiefly to the development and expansion, according to powers of m, of the divisors introduced by integration? In the latter case the difficulty might be easily avoided; but each of these questions must be answered in the negative."

In order to illustrate this point, the author has selected indifferently two terms in the longitude amongst those in which this divergence is met with, and he examines their construction without introducing details which do not bear immediately upon the point referred to.

After this examination he concludes the section, and with it the fourth part of his work On the Theory of the Moon, with the annexed observations:—

"These are the most considerable, but it would evidently be impossible to employ with safety any rule of approximation which did not embrace other terms. In this and in other cases in the lunar theory it will be found that coefficients, when formed correctly, are made up by the addition of numerous small terms, which come from various sources: hence the danger of attending only to the

leading or principal terms which may occur upon an incomplete examination: hence also the extreme practical difficulty of the problem in whatever manner it be approached.

"In the method which M. Plana has adopted, first, the mean longitude of the moon is obtained in terms of the true longitude, and the true longitude is afterwards found in terms of the mean longitude by reversion. But the divergence of the numerical coefficients exists equally in the former expression, and does not arise in the operation of reversion."

A note is appended "*On the Calculation of the Distance of a Comet from the Earth.*" Sir J. Lubbock having in his former Treatise on that subject (noticed in our fourth volume, p. 219) considered the position of the comet and of the sun to be defined by *longitude* and *latitude*, deduces, in this note, from the equations given in that treatise, those which must be employed if the positions of the comet and sun are defined by *right ascension* and *declination*; after which he successively examines the equations of Legendre and Lagrange (showing the former to be preferable), notices the equation recommended by Mr. Airy, and the solution of Laplace; and concludes as follows:—

"If observations of right ascension and declination be employed, the trouble of calculating the observed longitudes and latitudes is avoided, but additional terms are introduced explicitly into the final equations from which the distance of the comet is sought, and which may be considered as identical, whichever data are employed."

An Essay on Single Vision. By Dr. Woodhouse, Fellow of Caius College, Cambridge. Weale, 59, High Holborn, 1841.

This subject belongs to a class of questions intermediate between Natural Philosophy and Pathology, the research of which as a branch of science has been recommended by Bacon in his *Novum Organum.*

Suggestions on this particular question have been made by Newton, Harris, Wood, Herschel and other optical writers, but which cannot be considered as completely accounting for the phænomenon.

The author considers "that the parts immediately acted upon by any exciting power, do perceive." This position admits of dispute; it is well known that during the Taliacotian process, an injury inflicted on the new-made nose is felt on that part of the forehead from whence the skin had been removed (see Johnstone on Sensation). We have only room to notice two, out of many ingenious experiments by the author. Let C be an object looked at, and P one interposed. Two images of P will appear. If P be moved towards C, those images will mutually, and *equally* approach towards coalescing.

Exp. 2. Let a candle be placed a few feet from the observer, and let him look at a pencil between the candle and his eyes; two images of the candle will appear: but, contrary to the first experi-

ment, when the pencil is moved, one of the images remains stationary, the other proceeding alone to coalesce with it.

From similar experiments, and some physiological considerations, the Doctor infers that one eye may be regarded as a *master eye* wholly occupied with vision, the other being nearly or wholly passive. This tract will repay perusal.

The author concludes it with the following problem, which had been treated already by Wollaston, viz. " When a picture looks at you in one part of a room it will look at you in another." The author's solution seems simple and satisfactory.

XVI. *Proceedings of Learned Societies.*

LONDON ELECTRICAL SOCIETY.

June 15th.—A Letter was read from Martyn Roberts, Esq., Mem. Elec. Soc., describing " Some Experiments which show that Radiation, a property of Heat, is not a property of Electricity." The experiments were those in which a Bennet's electrometer is exposed to the influence of a charged conductor, and the leaves, which by induction had diverged, collapse the instant the charge is removed from the conductor. This would not be the case if electricity radiated as heat does: it would then pass through the interstices of the air and produce a permanent effect upon the instrument. The author in conclusion said, that " the establishment of facts such as these would throw much light upon the nature of electricity; and its dissimilarity from, or the analogy it bears to heat, will be one great means of penetrating more deeply into these mysterious agencies."

A translation was read of a paper entitled " On the Remarks of M. Becquerel relative to the comparative measure of the action of two voltaic pairs, one Copper and Zinc, the other Platinum and Zinc, by M. Jacobi." M. Becquerel had attributed the superior power of the nitric acid battery to the reaction between the nitric acid and the acidulated water. M. Jacobi considers this reaction by no means sufficient to produce the astonishing results obtained from a Grove's battery; and he alludes to some experiments by which M. Fechner had proved that of the whole power obtained from a battery, whose two exciting liquids were nitric acid and *alkali*, only $\frac{1}{60}$th part was due to this species of reaction; and hence M. Jacobi inferred that when *acidulated water* was substituted for alkali, the amount due to this reaction would be in proportion still less. The author here gave a series of propositions deduced from our present knowledge of voltaic phænomena; and concluded with some further experiments in illustration of the actual superiority possessed by the platinum over the copper battery.

We must refer our readers to the Proceedings of the Society for the other papers which were read: viz.—

" Descriptive Memoir of an Atmospheric Electrical Apparatus, and its appendages, comprehending an insulated line of wire, extending

about three hundred and sixty-five yards horizontally over the town of Sandwich," by W. H. Weekes, Esq.

A Letter addressed to J. P. Gassiot, Esq. from H. Coller, Esq., describing some experiments, not altogether unsuccessful, in copying a Daguerréotype Plate by electrotype.

The date of the experiments was March 1840. The Plates obtained were on the table.

"A Register of the electric state of the Atmosphere in relation to its Meteorological Phænomena, for the month of May 1841, from observations made with the insular apparatus already described," by W. H. Weekes, Esq.

This register, which will be supplied regularly, will be of great value on account of the mode of obtaining the data it contains.

The reading of a communication from Andrew Crosse, Esq., Mem. Elec. Soc., and another from Thomas Pine, Esq., was deferred to the next meeting.

LONDON INSTITUTION.

At a meeting of the Managers of the London Institution, on Friday, June 4th, Sir Thomas Baring in the Chair, Professor Grove showed the electric spark as produced by the condensation of effluent steam, the original experiments on which subject have already appeared in our journal. At one extremity of a brass insulated conductor was suspended a copper plate of about 7 inches diameter: the jet of steam issuing from a tube connected with the boiler of a model low-pressure steam-engine, impinged upon this plate, and several sparks of a quarter of an inch in length, were drawn from the conductor. The electricity of the conductor was shown to be positive. A Leyden phial was charged, &c. We understand that it is the intention of Professor Grove to repeat in the theatre of the Institution any novel scientific experiments which may be capable of popular illustration.

XVII. *Intelligence and Miscellaneous Articles.*

TWO LETTERS ON CALOTYPE PHOTOGENIC DRAWING, FROM H. F. TALBOT, ESQ., F.R.S., TO THE EDITOR OF THE LITERARY GAZETTE.

Dear Sir,—It is now two years since I first published a brief account of Photogenic Drawing. During this interval I have taken much pains, and made many experiments, with the hope of rendering the art more perfect and useful. In this way I have obtained a good many improvements, with the mention of which I shall not detain you at present.

I shall confine myself in this letter to a single subject, viz. the discovery which I made last September of a chemical process by which paper may be made far more sensitive to light than by any means hitherto known. It is not easy to estimate exactly how far this increase of sensibility extends; but certainly a much better picture can now be obtained in *a minute* than by the former process in *an hour.*

This increased rapidity is accompanied with an increased sharpness and distinctness in the outlines of the objects—an effect which is very advantageous and pleasing, and at the same time rather difficult to account for.

The shortest time in which I have yet succeeded in impressing an image in the camera obscura has been *eight seconds*; but I do not mean to assign this as the precise limit, for it can only be ascertained by more careful and multiplied experiments.

The production of the image is accompanied with some very extraordinary circumstances, to which I will advert in a subsequent letter. These phænomena are extremely curious; and I have not found in chemical writers any mention of anything similar.

The image, when obtained, must of course be fixed, otherwise the process would remain imperfect. It might be supposed, *à priori*, that this fixation would be very difficult, the paper being so sensitive. But it fortunately happens that, in this instance, what seems a reasonable inference is not borne out by fact, that new photographs are more easily and perfectly fixed than was the case with the former ones. When fixed, a great many copies may be made from them; and thus the original view can be multiplied with facility.

I think that the art has now reached a point which is likely to make it extensively useful. How many travellers are almost ignorant of drawing, and either attempt nothing, or bring home rude unintelligible sketches! They may now fill their portfolios with accurate views, without much expenditure of time or trouble; and even the accomplished artist will call in sometimes this auxiliary aid, when pressed for time in sketching a building or a landscape, or when wearied with the multiplicity of its minute details.

One of the most important applications of the new process, and most likely to prove generally interesting, is, undoubtedly, the taking of portraits. I made trial of it last October, and found that the experiment readily succeeded. Half a minute appeared to be sufficient in sunshine, and four or five minutes when a person was seated in the shade, but in the open air. After a few portraits had been made, enough to show that it could be done without difficulty, the experiments were adjourned to a more favourable season.

Several photographic processes being now known, which are materially different from each other, I consider it to be absolutely necessary to distinguish them by different names, in the same way that we distinguish different styles of painting or engraving. Photographs executed on a silver plate have received, and will no doubt retain, the name of Daguerréotype. The new kind of photographs, which are the subject of this letter, I propose to distinguish by the name of *Calotype*; a term which, I hope, when they become known, will not be found to have been misapplied.

I remember it was said by many persons, at the time when photogenic drawing was first spoken of, that it was likely to prove injurious to art, as substituting mere mechanical labour in lieu of talent and experience. Now, so far from this being the case, I find that in this, as in most other things, there is ample room for the ex-

ercise of skill and judgement. It would hardly be believed how different an effect is produced by a longer or shorter exposure to the light, and, also, by mere variations in the fixing process, by means of which almost any tint, cold or warm, may be thrown over the picture, and the effect of bright or gloomy weather may be imitated at pleasure. All this falls within the artist's province to combine and to regulate; and if, in the course of these manipulations, he, *nolens volens*, becomes a chemist and an optician, I feel confident that such an alliance of science with art will prove conducive to the improvement of both.

I remain, yours, &c.

31, Sackville Street, Feb. 5, 1841. H. F. TALBOT.

DEAR SIR,—I will now proceed to give you some further details, for which I had not room in my last letter, respecting the phænomena which occur during the very singular photographic process to which I have given the name of Calotype. And I may as well begin by relating to you the way in which I discovered the process itself. One day, last September, I had been trying pieces of sensitive paper, prepared in different ways, in the camera obscura, allowing them to remain there only a very short time, with the view of finding out which was the most sensitive. One of these papers was taken out and examined by candlelight. There was little or nothing to be seen upon it, and I left it lying on a table in a dark room. Returning some time after, I took up the paper, and was very much surprised to see upon it a distinct picture. I was certain there was nothing of the kind when I had looked at it before; and, therefore (magic apart), the only conclusion that could be drawn was, that the picture had unexpectedly *developed itself* by a spontaneous action.

Fortunately, I recollected the particular way in which this sheet of paper had been prepared, and was, therefore, enabled immediately to repeat the experiment. The paper, as before, when taken out of the camera, presented hardly anything visible; but this time, instead of leaving it, I continued to observe it by candlelight, and had soon the satisfaction of seeing a picture begin to appear, and all the details of it come out one after the other.

In this experiment, the paper was used in a moist state; but since it is much more convenient to use dry paper if possible, I tried it shortly afterwards in a dry state, and the result was still more extraordinary. The dry paper *appeared* to be much less sensitive than the moist; for when taken out of the camera after a short time, as a minute or two, the sheet of paper was absolutely blank.

But, nevertheless, I found that the picture existed there, *although invisible*; and by a chemical process analogous to the foregoing, it was made to appear in all its perfection.

After several further experiments, which were requisite in order to come to a right understanding of this unexampled natural process, I found it expedient to abandon the former method of taking views with the camera, in favour of the new one, so far excelling it in ra-

pidity and power. The result of my experience hitherto with this calotype paper is, that if properly prepared, it will keep three or four months, ready for use at any moment, and moreover it is used in a dry state, which is a great convenience.

The time of exposure to light in the camera may be varied, according to circumstances, from a quarter of a minute upwards; and the paper, when taken out of the instrument, appears quite blank, as I said before, but it is impressed with an invisible image. It may be kept in this invisible state for a month or so, if desired, and *brought out*, or rendered visible, when wished for. But, generally, this is done shortly after, or at least on the same day, for fear of accidents (such as a casual gleam of daylight, which would at once annihilate the whole performance). Whenever it is desired to render the picture visible, this is done in a very short time, as from a minute to five or ten minutes, the strongest impressions coming out the easiest and quickest. Very faint impressions (as those obtained when the paper has been only a few seconds in the camera, or the objects have not been luminous enough) take a longer time in coming out; but they should not be despaired of too soon, as many of them exhibit difficulty at first, as if reluctant to appear, but nevertheless end by coming out very well. The operator of course remains in a darkened room, lit by candles only.

I know few things in the range of science more surprising than the gradual appearance of the picture on the blank sheet, especially the first time the experiment is witnessed. The operator ought to watch the progress of the picture, until, in its strength of colour, sharpness of outline, and general distinctness, it has reached in his judgement the most perfect state. At that moment he stops further progress by washing it over with a fixing liquid. This is washed off with water, the picture is then dried, and the process is terminated.

The picture is found to be very strongly fixed, and from it numerous copies may be taken on common photogenic drawing paper, by the method of superposition in sunshine. The original picture does not readily become altered, or wear out by this exposure to the sun; but in case it does so, as happens sometimes, I find that it may be in general readily *revived*. This *revival*, which is a most curious particularity of the calotype process, not only restores the picture to its pristine strength, but frequently causes fresh details and minutiæ to appear in the picture, which had not appeared before, at the time when it was first *brought out*, or rendered visible (owing to that process having been checked too soon). These details, therefore, had been lying in an invisible state on the paper all this time, *not destroyed* (which is the most extraordinary thing) *by so much exposure to sunshine*. They were protected by the fixing liquid. But no one could have supposed beforehand, or without ocular demonstration, that it could have exerted so complete a protecting power. This is an invaluable property of the calotype—the power of reviving the pictures—not only because it allows so many copies to be made, but because it enables the artist to correct the error of his judgement, in case he has made too faint a picture at first, by stopping it too soon while it was coming out.

Some further details on this subject, and an account of the chemical processes employed, I reserve for a paper which I intend to lay before the Royal Society.

I am, &c. &c.

Lacock Abbey, Feb. 19, 1841, H. F. Talbot.

PROCESS FOR OBTAINING THE HYDROBROMIC AND HYDRIODIC ACIDS. BY DR. R. M. GLOVER.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the latest Report of the British Association, there is an account of a new process for obtaining hydrobromic and hydriodic acids, said to be communicated by me. This account is quite incorrect. I first observed this erroneous report in the Athenæum, whence its was copied into several British and Foreign journals; but as I gave a correct abstract to Dr. Playfair, one of the Secretaries to the Chemical Section, when he passed through this town shortly after the meeting, I expected to see the error corrected in the Report of the Association.

It is known that all the solid bromides and iodides are decomposed by sulphuric acid with evolution of bromine and iodine; but I find that although the solid bromide and iodide of barium obey the general rule, their solutions are decomposed by sulphuric acid without any evolution of the elements, and with the formation of pure solutions of the hydracids. This difference is doubtless owing to the agency of the water in speedily removing the sulphuric acid in the form of the insoluble sulphate. The Report of the Association incorrectly states that I had proposed the solid iodide and bromide of barium acted on by sulphuric acid as convenient sources of the hydracids. My process is the very reverse.

In this way, in particular, hydrobromic acid can be conveniently obtained; more conveniently indeed than the hydriodic acid; because it is difficult to prepare the iodide of barium unmixed with the carbonate of baryta and free iodine, while the bromide does not undergo any decomposition from the carbonic acid of the atmosphere. A saturated solution of the bromide may be decomposed by a solution of sulphuric acid diluted with an equal bulk of water, the diluted acid being allowed to cool before it is added to the saline solution, and hydrobromic acid is set free unmixed with bromine. In this way the amount of the hydracid in any solution of it we may employ can be easily known, calculating from the weight of the bromide decomposed.

Should you insert this rectification, you will oblige

Your obedient Servant,

Robert Mortimer Glover, M.D.,
Lecturer on Chemistry in the Newcastle School of Medicine.

Literary and Philosophical Society Rooms,
Newcastle, April 21, 1841.

NEW ALUM. BY DR. MOHR.

A variety of alum has been for some time introduced into Germany, which is recommended as containing the principles necessary to dyeing and calico printing in a state of great concentration; this quality is represented as rendering its use more advantageous, and less expensive in carriage. This alum has not the slightest resemblance to common potash alum; it exists in flat quadrangular tables, which are slightly transparent, and dissolve very readily in water; the taste of this salt is sweetish, acid, and aluminous, and much more distinctly so than common alum; when heated in a crucible it swells, and eventually becomes a gummy mass, which, when more strongly heated, gives out sulphuric vapours. Nevertheless the mass which has been heated to redness is entirely and readily soluble in water. If pulverized sulphate of potash be thrown into a concentrated solution of this alum, a crust of common alum is quickly formed. The usual reagents prove that it contains sulphuric acid, alumina, and a small quantity of potash, but no ammonia. M. Mohr found it to consist of

Sulphuric acid......	36·24	
Alumina...........	13·91	
Potash...........	1·50	
Water............	49·60	101·15

This composition shows that the alum in question is, properly speaking, merely sulphate of alumina with an excess of acid, and combined with water of crystallization. The small quantity of sulphate of potash shows the origin of this salt, it being probably prepared with pulverized and calcined pipe-clay and sulphuric acid not completely concentrated. This new alum is entirely free from iron. —*Journal de Pharmacie*, t. xxvi. p. 633.

ON THE SUPPOSED HYDRATE OF PHOSPHORUS. BY M. MARCHAND.

M. Pelouze was the first chemist who supposed that the white crust which covers phosphorus that has been long kept, is a hydrate of this substance: M. H. Rose regards it merely as a modification of the state of aggregation: M. Mulder believes that it is a compound of oxide of phosphorus and phosphuretted hydrogen. He has remarked that the white sticks of phosphorus become of a red colour in aërated distilled water, and he immediately obtained a similar white substance by subjecting red oxide of phosphorus to the action of phosphuretted hydrogen.

M. Marchand being desirous of settling the question, pressed a quantity of the white crust, strongly, between folds of filtering paper, as had been done by M. Rose; he then placed it in a capsule over sulphuric acid *in vacuo*, and allowed it to remain so for several days. On the first day a feeble light was perceptible; but it soon disappeared: and when access of air was allowed the mass inflamed as if it had been phosphorus under similar circumstances.

A fresh quantity of the white crust was again dried over sulphuric acid, but under a receiver without a vacuum. M. Marchand placed

it afterwards, as M. Rose had done, in a long weighed tube of glass, the lower end of which was closed. He ascertained the quantity, and immersed the tube in hot water. There was but little moisture disengaged, and it was very probably hygrometric, and the residue, which was converted into common phosphorus, was weighed, and found scarcely diminished.

In order to verify the assertions of M. Mulder, the author put white sticks of phosphorus into aërated water; he left them in it for several weeks without perceiving any alteration of colour; he then directed upon them a current of oxygen gas, without producing any sensible effect. He obtained, as M. Mulder did, the compound of oxide of phosphorus and phosphuretted hydrogen, and found that it strongly resembled the white crust; but the manner in which it was acted upon by heat, he found to be very different.

M. Marchand concludes, therefore, from his experiments, that the supposed white crust is merely, as was first supposed by M. Rose, phosphorus in an altered state of aggregation.—*Journal de Pharmacie*, tom. xxvi. p. 764.

NITRO-SACCHARIC ACID.

M. Boussingault states that this acid is obtained by dissolving sugar of gelatin in weak nitric acid. It is to be slightly heated, and on cooling the solution crystallizes; no other reaction takes place besides the mere dissolving of the sugar in the acid. This acid has a very sour taste, and also a slightly saccharine one. M. Boussingault analysed it in three different states: crystallized, dried, and as it exists in salts.

Dried at 230° Fahr., this acid contains,

	By experiment.	By calculation.	
Hydrogen	4·2	4·0	H^{42}
Carbon	18·1	18·2	C^{32}
Oxygen	56·5	56·3	O^{37}
Azote....	21·2	21·5	N^{16}
	100·	100·	

Nitro-saccharic of silver crystallizes very readily; this salt burns without detonating. Its composition is,

Hydrogen	1·86
Carbon................	10·08
Oxygen	27·63
Azote	11·83
Silver	48·60——100·00.

It is stated by the author that nitro-saccharic acid dried at 210° Fahr. loses four equivalents of water, which are replaced in the nitro-saccharate of silver by four equivalents of oxide of silver, and the analysis of the nitro-saccharate of potash leads to the same conclusion; it was also observed by M. Boussingault, that it will appear on slight examination that the nitric acid remains unmodified.

The nitro-saccharate may be represented as resulting from the union of nitric acid with the corresponding saccharate, or as a com-

pound of sugar of gelatin with a nitrate; and, in fact, the nitro-saccharates may be obtained by treating the saccharates with nitric acid.

The following are the formulæ of the various compounds of sugar of gelatin which have been mentioned:—

Sugar of gelatin	$C^{32} H^{36} N^{8} O^{14}$
,, ,, in salts............ ..	$C^{32} H^{30} N^{8} O^{11}$
,, ,, combined with silver	$C^{32} H^{30} N^{8} O^{11} (Ag\,O)^{4}$
,, ,, ,, ,, copper	$C^{32} H^{30} N^{8} O^{11} (Cu\,O)^{4}$
,, ,, ,, ,, lead	$C^{32} H^{30} N^{8} O^{11} (Pb\,O)^{4}$
Nitro-saccharic acid crystallized.........	$C^{32} H^{30} N^{8} O^{11} (N^{2} O^{5})^{4} (H^{2} O)^{9}$
Nitro-saccharic acid dried at 230° Fahr.....	$C^{32} H^{30} N^{8} O^{11} (N^{2} O^{5})^{9} (H^{2} O)^{3}$
,, ,, in salts	$C^{32} H^{30} N^{8} O^{11} (N^{2} O^{5})^{4} (H^{2} O)^{2}$
Nitro-saccharate of silver	$C^{32} H^{30} N^{8} O^{11} (N^{2} O^{5})^{4} (Ag\,O)^{4} (H^{2} O)^{2}$
,, ,, of potash	$C^{32} H^{30} N^{8} O^{11} (N^{2} O^{5}) (K\,O)^{4} (H^{2} O)^{2}$.

Journal de Pharmacie, t. xxvii. p. 37.

Preparing for Publication.

The Geology and Mineralogy of Engineering: including the Principles of the sciences of Œconomic Geology and Mineralogy as applied to the Arts. By Edward William Brayley, Fellow of the Linnæan and Geological Societies, Associate of the Institution of Civil Engineers, Corresponding Member of the Royal Geological Society of Cornwall, &c.

METEOROLOGICAL OBSERVATIONS FOR MAY 1841.

Chiswick.—May 1. Fine: very hot: clear. 2. Very fine: cloudy. 3. Rain. 4. Foggy: cloudy and fine: very heavy fall of rain at night. 5. Rain: cloudy and fine: lightning at night. 6. Rain: cloudy. 7. Fine: rain. 8. Heavy showers. 9, 10. Very fine. 11. Overcast: slight rain at night. 12. Cloudy and fine. 13. Fine. 14—16. Very fine. 17. Cloudy and windy. 18. Fine. 19. Rain with strong wind. 20. Boisterous: showery: cold at night. 21. Dry haze: rain. 22. Showery and mild. 23. Slight haze: cloudy and fine. 24. Fine. 25. Very fine. 26. Hot and dry. 27. Sultry: much sheet-lightning at night, with occasionally some of the zigzag and forked kind, together with thunder, and abrupt showers falling in large drops. 28. Sultry: very fine. 29. Very fine: lightning at night. 30, 31. Overcast and very fine.

Boston.—May 1. Fine: therm. 66° half-past 2 P.M. 2. Cloudy: rain with thunder and lightning P.M. 3. Cloudy. 4. Cloudy: rain early A.M. 5. Rain. 6. Cloudy: rain early A.M. 7. Cloudy: rain P.M. 8. Rain. 9. Fine. 10. Cloudy. 11. Fine: therm. 72° 3 o'clock P.M.: rain P.M. 12. Cloudy. 13. Fine. 14. Cloudy. 15. Fine: therm. 69° 3 o'clock P.M. 16. Fine. 17. Cloudy: rain P.M. 18. Cloudy. 19. Rain: stormy P.M. 20. Stormy: rain A.M. and P.M. 21. Fine. 22. Cloudy: rain early A.M. 23. Cloudy. 24—27. Fine. 28. Cloudy: therm. 79° 3 o'clock P.M.: rain P.M. 29. Fine. 30. Cloudy. 31. Fine. N.B. This May month has been warmer than any preceding May month since 1834.

Applegarth Manse, Dumfries-shire.—May 1. Fair and fine: thunder. 2. Drizzling afternoon. 3. Fair: frosty: hail. 4. Rain all day. 5. Rain occasionally. 6. Fair and fine. 7. Rain for four hours. 8. Rain P.M.: thunder. 9. Fair till night: rain P.M. 10. Fair but cloudy. 11. Wet A.M.: cleared up. 12—15. Fair and fine. 16, 17. Wet nearly all day. 18, 19. Showers A.M., then fine. 20. Showers. 21. Fair and fine. 22. Wet A.M.: fine P.M. 23. Fair but gloomy. 24. Fine summer day. 25, 26. Bright and cool. 27. Parching wind and hot sun. 28. Fine: rained a little. 29. Fair and fine. 30. Soft rain from eleven to four. 31. Fine summer day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. May.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Wind. London: Roy.Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy.Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
1.	30·040	29·998	29·794	29·44	29·90	29·68	57·8	67·2	47·0	76	42	58	65	40	NE.	E.	calm	SW.	...	...	...	...	52
2.	29·720	29·674	29·640	29·00	29·56	29·60	61·7	70·4	53·2	76	45	61	55½	45½	S.	SW.	calm	SE.	...	·25	...	...	57
3.	29·772	29·780	29·716	29·18	29·66	29·66	47·7	71·5	47·2	50	43	49	51	32½	N.	NE.	calm	SE.	·375	·30	·36	...	52
4.	29·628	29·609	29·480	29·13	29·48	29·25	53·7	54·8	45·3	69	50	46	51	40	E.	S.	SE.	SE.	·250	·70	·04	...	49
○ 5.	29·464	29·571	29·399	28·85	29·15	29·25	55·4	66·0	52·0	62	46	53	59½	49½	SE.	SW.	calm	SW.	·719	·15	·25	...	55
6.	29·556	29·774	29·495	28·91	29·31	29·42	57·8	61·3	53·0	65	43	57·5	55½	45½	S.	SW.	W.	S.	·077	·02	·08	...	55
7.	29·746	29·708	29·528	29·18	29·50	29·38	59·2	64·6	49·7	66	47	53	58	45	S.	S.	calm	SW.	·019	·10	...	...	56
8.	29·548	29·581	29·491	28·92	29·38	29·55	57·5	66·6	52·5	62	44	54	59	35	S.	SW.	W.	SSW.	·116	·16	·36	0·80	54
9.	30·094	30·097	30·008	29·44	29·75	29·80	56·3	72·2	47·3	65	50	56·5	55	37	S.	SW.	W.	SW.	·044	...	·02	...	52
10.	30·180	30·153	30·117	29·50	29·73	29·80	60·2	63·3	53·0	65	46	59	56	46	S.	SW.	W.	WSW.	...	...	...	...	53
11.	30·094	30·054	30·003	29·45	29·75	29·87	62·2	63·8	51·6	77	46	64	56½	50½	S.	S.	SW.	S.	...	·01	...	...	55
12.	30·190	30·247	30·128	29·55	30·06	30·19	57·0	70·7	53·0	62	39	55·5	57	40	NNW.	NE.	calm	WSW.	·019	...	·05	...	56
☾ 13.	30·388	30·330	30·314	29·77	30·25	30·26	55·7	63·4	47·5	66	36	58	60½	39	N.	NE.	calm	SW.	...	...	...	...	52
14.	30·424	30·364	30·262	29·83	30·23	30·11	53·7	63·6	46·3	65	40	55	62½	43½	NE.	NE.	calm	SW.	...	...	...	...	49
15.	30·244	30·194	30·043	29·59	30·00	29·84	59·7	69·4	48·7	74	40	61	59	44	S.	W.	calm	S.	...	...	...	...	52
16.	29·968	29·911	29·674	29·24	29·60	29·49	53·7	66·0	49·0	74	49	60	56½	46½	SSW.	SW.	W.	WSW.	...	...	...	0·64	50
17.	29·664	29·632	29·518	28·93	29·20	29·10	58·3	70·0	52·3	68	45	55·5	56	46½	S.	SW.	SW.	SW.	...	·01	...	...	55
18.	29·610	29·553	29·531	28·90	29·15	29·26	57·8	63·0	49·4	66	46	57	56½	46	S.	SW.	W.	SW.	...	·02	·02	...	54
19.	29·406	29·674	29·280	28·77	28·98	28·85	55·3	65·7	51·3	60	47	52	58	45	S.	S.	NW.	S.	...	·15	·06	...	53
● 20.	29·328	29·578	29·253	28·65	28·83	29·20	58·2	59·6	50·7	61	39	54	56½	45	W.	SW.	SW.	W.	·075	·06	...	...	53
21.	29·702	29·647	29·526	29·19	29·51	29·64	58·7	67·7	48·4	69	54	57·5	59	43½	ENE.	E.	calm	SW.	·044	·16	·09	...	52
22.	29·672	29·811	29·607	29·10	29·70	29·80	59·7	65·8	56·5	68	43	55	61	48	N.	S.	calm	SW.	·133	·01	·02	...	54
23.	30·040	30·159	29·994	29·45	29·90	30·05	58·3	64·2	53·7	75	46	56	61	49	S.	SE.	calm	E.	...	...	...	...	54
24.	30·242	30·180	30·137	29·61	30·10	30·10	63·5	70·3	53·6	70	51	65	65½	49	NE.	E.	calm	SW.	...	...	...	...	55
25.	30·220	30·165	30·133	29·69	30·25	30·27	63·5	70·6	55·8	73	54	65·5	64	43	N.	NE.	E.	E.	...	...	...	...	61
26.	30·146	30·070	29·955	29·58	30·17	30·03	67·7	70·0	55·0	80	58	61	67	46½	NE.	NE.	E.	E.	...	...	...	...	62
☽ 27.	29·960	29·882	29·823	29·35	29·95	29·85	71·7	79·4	62·0	82	60	69	78	51	NE.	NE.	calm	NE.	...	·03	...	...	66
28.	30·000	30·038	29·893	29·20	29·80	29·90	70·3	79·0	64·8	80	51	68	72	50	SSE.	SW.	calm	SW.	·116	...	...	1·80	65
29.	30·186	30·096	30·061	29·48	30·00	30·00	65·7	78·3	57·7	72	54	67	63	52½	NW.	N.	calm	SW.	...	·03	·05	...	55
30.	30·078	30·016	29·979	29·44	30·02	29·95	61·5	71·9	58·0	74	49	65·5	63	42	NW.	NW.	N.	SW.	...	...	...	...	59
31.	30·086	30·042	29·995	29·43	29·78	29·90	66·7	78·6	57·3	78	49	66	68	42	S.	W.	W.	SE.	...	...	...		62
Mean.	29·916	29·922	29·799	29·28	29·700	29·711	59·6	68·0	52·4	69·35	46·83	58·5	60·2	44·4					Sum. 1·987	2·16	1·42	3·24	Mean. 55

THE

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[THIRD SERIES.]

AUGUST 1841.

XVIII. *On some Electro-Nitrogurets.* *By* W. R. Grove, *Esq. M.A. F.R.S.**

THE ammoniacal amalgam, or that extraordinary substance which is formed by electrolysing solutions of ammonia or its salts with mercury at the cathode, first discovered by Dr. Seebeck, has at various periods attracted the attention of the most distinguished chemists, and from being the apparent link between the metals and permanent gases, must ever be an object of deep interest. The experiments which I here venture to lay before the Royal Society, will be found, I trust, to throw some light upon this compound, to connect the ammoniacal amalgam with the experiments performed by different philosophers upon the decomposition of ammonia during its passage over heated metals, to explain some anomalies in these effects, and to make known some new and curious combinations.

The first experiment which I made on the ammoniacal amalgam was to freeze it, which, by the kindness of Mr. Everett, I was enabled to do in the laboratory of the Middlesex Hospital, and at which experiment Professor Schönbein, of Basle, was present. A mass of the compound formed by immersing an amalgam of potassium in muriate of ammonia was subjected to solid carbonic acid liquefied in the usual way by sulphuric æther; it froze readily, but at what exact temperature we had no means of ascertaining; at the moment of solidifying it contracted slightly, but without giving out any gas; in its

* Communicated by the Author; having been read before the Royal Society, on the 4th of February, 1841.

solid state it could be broken, and its fracture was of a very dark gray, or nearly black colour, something resembling that of cast iron which has been exposed for some time to the air; it had little or no metallic lustre: on thawing it gave out ammonia, hydrogen and nitrogen.

My next attempt to procure a solid ammoniacal compound of this nature, was, by forming an extremely fusible alloy of mercury, and Newton's fusible metal. I obtained one which was solid at ordinary temperatures, but which fused at 86° Fahrenheit. I made this the cathode of a voltaic battery in a solution of ammonia gently heated; it remained perfectly unaltered, giving off the gases as a solid electrode would have done. It now occurred to me that by subjecting a solution of one of the salts of ammonia to electrolysis, having at the anode an oxidable metal; this would be dissolved and revived at the cathode in conjunction with nascent nitrogen and hydrogen, and thus a permanent compound might be formed; to effect this it was necessary that the oxide or chloride of the metal should be easy of reduction, soluble in, and not precipitable by, ammoniacal salts; thus lead, iron, and platina offered little hope of success, and on trial proved ineffectual. Tin was also inefficient from the quantity of insoluble peroxide and oxychloride formed; with this metal indeed a small quantity of dark-gray spongy matter collected at the cathode, but was soon reacted upon by the solution. With zinc, copper and cadmium, however, the results were very different; and the compounds formed and methods of forming them, I will describe separately. A long glass trough was filled with distilled water, into which were placed several lumps of muriate of ammonia; a platina wire forming the cathode, and a button of distilled zinc suspended by a platina wire, the anode of six pairs of zinc and platina plates (charged, as originally proposed by me (Phil. Mag. for May 1839), with dilute sulphuric and concentrated nitric acid,) were immersed in the liquid at the two extremities of the trough, and separated by a fence of crystals of the salt. A spongy mass, which appeared to be formed of a cluster of folia or irregular facets, of the colour and lustre of plumbago, soon formed around the cathode, and as it increased in size rose up and floated; this specific levity was, however, principally due to the gas entangled in its interstices, as when slightly compressed between two strips of glass it instantly sank; having formed a sufficient quantity of this sponge it was gently removed, placed on a filter, and well washed with distilled water, then dried over a sand-bath at a gentle heat, during which it gave out no ammoniacal odour; then pulverized by

rubbing between the folds of filtering paper; when thus dried and reduced to powder its colour was unaltered; it had no metallic lustre: the specific gravity of a portion of it which I tried, proved to be 4·6; it conducted electricity, and was acted on by acids precisely as metallic zinc, but the hydrogen evolved from it by dilute sulphuric acid was mixed with a little nitrogen; when this solution was supersaturated with caustic potass it evolved no ammoniacal odour.

Five grains of the substance well dried, were placed in a narrow tube bent so as to form a retort, and heated to redness by a spirit-lamp; they gave out a permanent gas, which was at first collected over recently boiled mercury, but as no ammonia was detected, the gas in subsequent experiments was collected over water: the following is the result of four experiments, the air in the tube having been first rarefied and a portion expelled by heating it at parts distant from the substance:—

Experiment.		Cubic inches.
1st	5 grains gave	0·7
2nd	———	0·75
3rd	———	0·76
4th	———	0·72
		4)2·93(0·73

This gas was examined by detonation in an eudiometer, which differed from those of Volta and Henry in being furnished with a loop of platina wire, which could be ignited at pleasure by a feeble voltaic current; this eudiometer I have found to possess many advantages, but it would be out of place here to dilate upon them. The gas expelled from the zinc compound, when mixed with one-eighth of oxygen, detonated, and contracted to five-eighths of the whole volume; the residue suffered no further contraction by oxygen or hydrogen, and exhibited all the negative characters of pure nitrogen: upon several subsequent examinations portions of the original gas were found to contain from one-third to one-fourth hydrogen, and on examining the very last portion, or that which remained in the retort, I found it to leave a very small residuum when detonated with oxygen; the last portions of gas given out contained a greater proportion of hydrogen than the first, it thus appearing that the nitrogen was evolved at a lower temperature than the hydrogen; the residuum in the tube was unaltered as to colour, but contracted in bulk, and a little dew had collected in the bend of the tube.

Cadmium was subjected to the same process as zinc; its

electrolysis differed somewhat from that of zinc; with the latter metal the solution was during the whole time perfectly transparent, and no precipitate formed; with cadmium, after a short time, the hydrated oxide was precipitated by the ammonia formed at the cathode; but the portions of this precipitate contiguous to the cathode, being redissolved by excess of ammonia, were revived by the voltaic current; the sponge formed very rapidly; its growth could be distinctly seen; it was of a very dark leaden gray approaching to black, and resembled very closely the mercurio-ammoniacal amalgam; it had not the semi-crystalline appearance of the zinc sponge, but when pressed between glass had a leaden lustre and very much the appearance of an amalgam; if left in the solution, it was observed to contract and acquire a lighter shade of colour, most probably from the reaction of dissolved chlorine; the portions to be examined were, therefore, removed instantly and plunged into distilled water. The specific gravity was 4·8, analysed in the same way with the product from zinc; four experiments gave of permanent gas,

	Cubic inches.
1	0·25
2	0·18
3	0·20
4	0·20
	4)·83
Mean	·207

This gas, however, when examined, proved to contain no hydrogen; the residue in the tube was of a yellowish green colour mixed with globules of distilled cadmium.

Copper subjected to similar electrolysis produced a bright blue transparent solution, in which, at the cathode, grew a beautiful purple or rather chocolate-coloured coagulum; this frequently, but not always, floated upon the liquid; its specific gravity was 5·9. Five grains, when heated, gave of permanent gas,

Cubic inches.
0·13
0·10
0·09
0·11
4) ·43
·107

This gas also contained no hydrogen, but was wholly nitrogen;

the residue in the tube had changed colour from chocolate to red, and had all the characters of pure copper.

I now tried nickel and silver with the nitrate of ammonia and gold with the muriate; with nickel very little spongy matter was formed; I could not collect sufficient for analysis, and the fearful nature of the ammoniacal precipitate prevented my collecting sufficient of the silver deposit. To obtain an analogous compound of gold I mixed its chloride with a solution of muriate of ammonia, and electrolysed with platina electrodes, divided by a pipe-clay diaphragm, to prevent the reaction of the chlorine; a black deposit was formed, which did not float, and was but little coherent; its specific gravity was 10·3, and five grains gave 0·05 cubic inch of gas; from the small quantity obtained I could not positively pronounce as to its nature, but it appeared to be nitrogen. I have also formed amalgams by using mercury at the cathode with zinc, &c. at the anode, but the mercury, even when alloyed, would not retain the gases, and the amalgam yielded no more than the solid metal alone would have done.

In forming each of these substances the process must be continually watched, as, if the solution of the metal be in excess with respect to that of the ammoniacal salt, the metal is reduced alone; this is easily noted in the case of copper from the difference of colour; at a certain stage of the process the chocolate coagulum becomes fringed with dendritic fibres of pure copper, and the line of demarkation is clearly visible; in the zinc and cadmium compounds this effect is not so evident, but a very little practice enables one to recognize the difference, the pure metal being of a lighter gray, and assuming a regular arborescent appearance instead of the amorphous sponge; it generally commences on the under part, from the greater specific gravity of the metallic solution; when this reduction of pure metal has once commenced it continues, and to form again the nitroguret, the solution must be changed, and the process commenced *de novo.*

In all these compounds the quantity of nitrogen is far under the equivalent proportion, and in many cases gradations of shade could be observed in the sponge, on which occasions less nitrogen was found to have entered into combination; these gradations were somewhat abrupt, the mass having a stratified appearance like clay of different shades rolled up; the averages I have given were from portions of the deepest colour; an increase of power did not deepen it beyond a certain point; but whether a very high power will cause more nitrogen to combine with the metals, I have as yet had no opportunity of proving.

From the specific gravity of the cadmium compound I expected it to have yielded more nitrogen, and from the yellow colour of the residue I consider that some of this element, with probably hydrogen, formed a new combination analogous to that of ammonia and potassium examined by Sir Humphry Davy; to enter upon this analysis will require a new series of experiments, foreign to the immediate purpose of this paper. Another question of difficult resolution is, whether the hydrogen expelled from the zinc compound was in immediate combination, or resulted from the reaction of the zinc upon combined water: I incline to the opinion that the latter was the case, but see no means of proving it, as the same heat which would expel the water would decompose the substance. Although I had no expectation of procuring a solid compound (the theoretical ammonium), yet I did not neglect the trial; I distilled in the vapour of naphtha some of the zinc compound, but obtained only gaseous products. It may be worth remarking, that the quantities of nitrogen which enter into combination with the metals in these experiments is in proportion to their affinity for oxygen, and it may, perhaps, be considered an argument, in addition to the many already advanced, to prove that nitrogen is an oxide.

Independently of their individual interest, the strong analogy in character, and the manner of formation between these compounds and that of the ammoniacal amalgam, is (to me at least) satisfactory evidence of similarity of constitution. Berzelius supposed this to be an amalgam of mercury and ammonium. Gay-Lussac and Thenard supposed it to be a combination of nitrogen, hydrogen, ammonia and mercury. Mr. Daniell has advanced an opinion, supported by some experiments, that it is not a chemical combination, but a mere heterogeneous adhesion, the mercury being puffed up as a soap-bubble. If the above experiments entitle me to advance an opinion without incurring the charge of presumption, I should say that it was certainly a chemical combination, possibly of mercury, nitrogen and hydrogen, but more probably of mercury and nitrogen swelled with hydrogen; and from the circumstance of its being always moist, containing necessarily a good deal of ammonia, that its non-permanence is due solely to the mobility of the mercury; for place this *quatuor pedibus* with the other metals, i. e. solidify it, and the compound is perfectly permanent; reduce the other analogous metallic compounds to the state of the mercurial one, i. e. fuse, or even heat them, and they cease to be permanent. With regard to the hypothesis of ammonium, have we a right to assume the existence of a metal because the

nitroguret or ammoniuret of mercury bears a physical resemblance to an amalgam, might we not, upon the same principle, assume many other elements? As a theoretical question in the mind of an experimenter, it can do no harm; but as a recognized base, I cannot but think it may tend to mislead.

Another class of facts explained by the experiments above detailed, are those exhibited in the decomposition of ammonia during its passage over heated metals, first observed by, I believe, Berthollet, who remarked, that iron subjected to this process became brittle, but did not change in weight. Thenard, in similar experiments with iron, copper, silver, gold and platina, could not detect an increase in weight of above $\frac{1}{300}$th; Savart observed an increase of about $\frac{1}{300}$th. Despretz, however, although in his first experiments he could get no greater increase than Savart, yet on a repetition found that in the case of iron, and iron only, he could increase its weight 11 per cent.; he does not state the reason of his first failures, or of his subsequent success.

These somewhat discordant results appear to receive an easy explanation by my experiments, an explanation, indeed, first theoretically suggested by Ampère, viz. that in passing heated ammonia over the metals a nitroguret was formed and immediately decomposed; that thus the physical structure of the metals was changed without any notable increase of weight or chemical alteration.

Before concluding this paper, I ought not to omit a passage in one of Mr. Daniell's papers, which, upon a search made subsequent to my experiments, is the only one I have found bearing upon them. After stating some results with his constant battery charged with muriate of ammonia on the zinc side of the diaphragm, and sulphate of copper on the copper side, he makes the following remark:—

"There were no indications of free ammonia in the exterior cell; the precipitated copper, however, did not exhibit the beautiful, bright, pink hue which it ordinarily presents, but was of a dull, grayish, earthy appearance, resembling that of copper over which ammoniacal gas has been passed at a red heat, and probably contained some combined nitrogen. I had not, however, time to examine a compound which is worthy of further investigation."

All the copper nitrogurets which I have formed have had a chocolate colour, and the nearest substance to which I can liken them is the coagulum of the blood; the grayish colour remarked by Professor Daniell proceeded probably from some zinc being precipitated with the copper, or possibly the temperature at which he wrought (124°) occasioned some dif-

ference. With the exception of the above sentence I have found no anticipation of my results, and as far as my own knowledge and inquiries extend, am justified in giving them as new.

XIX. *On some supposed forms of Lightning.* *By* Michael Faraday, *D.C.L., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE magnificent display of lightning which we had on the evening of the 27th of last month, and its peculiar appearance to crowds of observers at London, with the consequent impressions on their minds, induces me to trouble you with a brief letter on certain supposed appearances and forms of lightning, respecting which the judgment of even good observers is often in error.

When, after a serene sky, or one that is not overcast, thunder-clouds form in the distance, the observer sees the clouds and the illumination of the lightning displayed before him as a magnificent picture; and what he often takes to be forked lightning (i. e. the actual flash, and not a reflexion of it), appears to run through the clouds in the most beautiful manner. This was the case on that evening to those who, being in London, observed the storm in the west, about nine o'clock, when the clouds were at a distance of twenty miles or more; and I have very frequently observed the same effect from our southern coasts over the sea. In many of these cases, that which is thought to be the electric discharge is only the illuminated edge of a cloud, beyond and behind which the real discharge occurs. It is in its nature like the bright enlightened edge which a dark well-defined cloud often presents when between the sun and the observer; and even the moon also frequently produces similar appearances. In the case of its production by lightning and distant clouds, the line is so bright by comparison with the previous state of the clouds and sky, so sudden and brief in its existence, so perfectly defined, and of such a form, as to lead every one at the first moment to think it is the lightning itself which appears.

But the forms which this line assumes, being dependent on the forms of the clouds, vary much, and have led to many mistakes about the shape of the lightning flash. Often, when the lightning is supposed to be seen darting from one cloud to another, it is only this illuminated edge which the observer sees. On other occasions, when he was sure he saw it ascend,

it was simply this line more brilliant at its upper than at its lower part. Some writers have described curved flashes of lightning, the electric fluid having parted from the clouds, gone obliquely downwards to the sea, and then turned upwards to the clouds again: this effect I have occasionally seen, and have always found it to be merely the illuminated edge of a cloud.

I have seen cases of this kind in which the flash appeared to divide in its course, one stream separating into two; and when flashes seen at a distance are supposed to exhibit this rare condition, it is very important the observer should be aware of this very probable cause of deception.

I have also frequently seen, and others with me, a flash having an apparently sensible duration, as if it were a momentary stream, rather than that sudden, brief flash which the electric spark always presents, whose duration even Wheatstone could not appreciate. This I attribute to two or three flashes occurring very suddenly in succession at the same place, or nearly so, and illuminating the same edge of a cloud.

The effect I have described can frequently be easily traced to its cause, and when thus traced best prepares the mind to appreciate the mistakes it may lead, and has led, to in the character, shape and condition of the lightning flash. It often happens at the sea-side, that, after a fine day, clouds will towards evening collect over the sea on the horizon, and lightning will flash about and amongst them, recurring at intervals as short as two or three seconds, for an hour or more together. At such times the observer may think he sees the lightning of a flash; but if he waits till the next illumination, or some future one, takes place, he will perceive that the flash appears a second time in the same place, and with the same form; or perhaps it has travelled a little distance to the left or right, and yet has the same form as before. Sometimes an apparent flash, having the same shape, has occurred three or four times in succession; and sometimes it has happened that a certain shaped flash having appeared in a certain place, other flashes have appeared in other places, then the first has reappeared in its place, and even the others again in their places. Now in all these cases it was simply the illuminated edges of clouds that were seen, and not the real flashes of lightning. These forms frequently appear to be in the cloud, and yet are not distinguishable till the lightning occurs. It is easy, however, to understand why they are then only developed, for that which appears in the distance to be one dull mass of cloud, distinguishable in figure only at its principal outline, often consists of many subordinate and well-shaped masses,

which, when the lightning occurs amongst or beyond them, present forms and lines before unperceived.

The apparent duration, which I before spoke of, is merely a case of very rapid recurrence, and may, by a careful observer, be easily connected with that which I have now proposed as the best test of the nature of the phænomenon.

There are some other circumstances which will help to distinguish the effect I have thus endeavoured to describe from the true appearance of the lightning flash, as the apparent thickness, sometimes, of the supposed flash, and its degree of illumination; but I have, I think, said enough to call attention to the point; and, considering how often the philosopher is, in respect to the character of these appearances, obliged to depend upon the report of casual observers, the tendency of whose minds is generally rather to give way to their surprise than to simplify what may seem remarkable, I hope I have not said too much.

I am, Gentlemen, your obedient servant,

June 22, 1841. M. Faraday.

XX. *Remarks on* Professor Poggendorff's *Paper on the Intensity of Current of the Zinc-iron Circuit. By* Martyn J. Roberts, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

CIRCUMSTANCES preventing my having access to your Magazine, it was long after the publication of your 114th Number that my attention was called to it as containing a translation of Prof. Poggendorff's Memoir, read before the Berlin Academy, upon the discovery made by me of an anomalous condition of iron in the galvanic circuit*.

In this memoir the learned Professor states that I have offered no opinion in explanation of the phænomenon, and that indeed a satisfactory one could hardly be given with the present views of electrical science in England. I must confess myself to blame in not having given the experiments in greater detail, as it might have prevented the mistake the learned Professor has evidently fallen into as to the cause of the phænomenon, and I trust the condition of electrical science in England is not so low as to prevent our giving an explanation of so simple a matter; indeed I am surprised that in repeating my experiments the learned Professor did not at once perceive the true solution of the apparent anomaly.

In his memoir he appears to attribute the superiority of the *iron-zinc* over the *copper-zinc* galvanic pair to some in-

* Phil. Mag. Third Series, vol. xviii. p. 42.

herent power of resistance to the reception of electric fluid, existing in a greater degree in copper than in iron; and if I understand him rightly, he supposes this to be not a less facility of conduction in the copper, but a resistance to the entrance of the electric fluid into the substance of the metal from another conducting medium in contact with it; that is to say, it is more difficult for electricity to enter copper from an acidulated solution in which this metal is immersed, than to enter iron in like circumstances, although the copper is actually a better conductor than iron when the electric fluid has once penetrated its substance. This I grant to be the fact, but not that the cause is any such singular and contradictory property in copper as first to offer a greater and then a less resistance to the passage of electricity; but it is simply that copper, when immersed in an acidulated solution, does not retain so clean a metallic surface as does iron when exposed to a like action.

When a *copper-zinc* pair is placed in dilute sulphuric acid an action takes place upon both the metals, and the balance of their affinities for the acid determines the direction and intensity of the electric current; but an obstacle to its free circulation arises by the resistance offered to its passage from the acid into the copper, because this metal has in a measure been acted upon by the acid and its surface partially oxidated; but as the affinity of the base for the acid under these circumstances is not sufficient to cause the solution of the oxide, it therefore remains upon the surface of the copper plate, and as oxides are worse conductors of electricity than their metallic bases, we have here a resistance presented by the oxidated surface to the entrance of the electric current into the copper plate. On the other hand, when an *iron-zinc* pair is immersed in dilute acid, we have also an action on both metals, but the balance of affinities is here not so much in favour of the zinc as when it is in combination with copper, therefore the *intensity* or *electromotive force* generated by the iron-zinc is not so great as in the copper-zinc battery; but as I have shown, the *quantity* circulated by the iron-zinc is greater, because the surface of the iron not only oxidates as did the copper, but in consequence of its greater affinity for the acid this oxide becomes dissolved in the liquid, and it is thus removed from the surface of the metal, which remains purely metallic, bright, and far more fitted to conduct electricity than would be the oxidated surface of a copper plate; it therefore offers less resistance to its entrance, and a larger *quantity* is thus circulated, although (in consequence of the balance of affinities) in less *intensity* or *electromotive force* by an iron-zinc than by a copper-zinc galvanic pair.

I think this explanation is proved to be the true one, both by Prof. Poggendorff's experiments and by my own. I found,—1st, When comparing a ten-pair iron-zinc with a ten-pair copper-zinc battery, that although in a given time of considerable duration a much greater quantity of water was decomposed by the iron than by the copper battery, yet that at the first immersion of the plates in acid a much more rapid decomposition was effected by the copper than by the iron combination, although in a very short time this superiority disappeared, and the iron became the strongest.

2ndly. When a single pair, copper-zinc, was tried against a similar pair, iron-zinc, and their powers tested by a differential galvanometer; at the first immersion the deviation was in favour of the copper, but in a short time the iron overcame it by a deviation of fifty degrees.

3rdly. When these pairs were inspected at intervals, it was seen that the iron preserved a bright metallic surface, while the copper was covered by a dark coat of oxide.

4thly. A copper-zinc pair was tested against an iron-zinc pair of equal surface, to ascertain the comparative loss of metal by oxidation and solution during the working of these little batteries, which were allowed to remain in action for about twelve hours; result as follows:—

	Original Weight.			Weight after 12 Hours' Action.			Loss.		
	drs.	s.	grs.	drs.	s.	grs.	drs.	s.	grs.
Iron . .	9	0	2	8	2	16	0	0	6
Zinc . .	6	1	10	5	1	8	1	0	2
Copper .	4	0	17	4	0	17	None.		
Zinc . .	6	0	17	4	2	17	1	1	0

showing that the copper loses none of its weight when in communication with zinc; but that iron lost six grains under similar circumstances.

From these experiments we may conclude, that as at the first immersion the copper-zinc is superior to the iron-zinc, but afterwards becomes inferior to it, some change must have taken place during this interval which reduces the electric current of the copper-zinc battery; this reduction is proved by the first and second experiments; what the change is was seen by inspection in the 3rd experiment, viz. a clean surface of iron, and an oxidated surface of copper. Lastly, that this bright iron surface is caused by the solution of the oxide of the metal, is proved by the 4th experiment, where the iron lost six grains of its weight.

The cause, then, of the anomalous electric condition of iron is not that this metal possesses a less power of resistance to the transition of electric fluid than does copper, although in

reality a worse conductor than this metal, a contradiction which has been maintained by Prof. Poggendorff; but it is, that iron, when in galvanic action, retains a more bright and metallic surface than copper does in like circumstances. This is also the case with all metals possessing the apparently anomalous property of being positive to a metal A, when associated with it as a galvanic pair; but more highly negative, or at least giving a stronger electric current than this metal A would, if associated with a metal B, still more positive than either of the former. I may call the attention of experimenters to the œconomy and power of the *iron-zinc* battery, which, by a simple contrivance to ensure a regular supply of acid, can be made as constant in its action as would be desired for ordinary purposes.

Compared with the *copper-zinc* battery, the following results show its power and œconomy:—

A small ten pair *iron-zinc* tested against a similar sized *copper-zinc* battery: an equal measure of dilute acid was poured into each battery; both were employed in decomposing water until all action ceased. The iron-zinc produced, by the expenditure of this acid, four cubic inches of mixed gases in 104 minutes. The copper-zinc produced, by a like expenditure of acid, only one and a half cubic inches of mixed gases, and this required 125 minutes. The expenditure of zinc is also much less when associated with iron than with copper, and this in the proportion of twenty to twenty-seven.

Prof. Poggendorff states, that it is necessary to give a zinc-platina battery three times as large a surface as that of a zinc-iron one to produce equal effects; and as zinc-platina has hitherto been considered the best galvanic arrangement, with the exception of Daniell's, the œconomy of the zinc-iron battery is manifest.

I am, &c.,

MARTYN J. ROBERTS.

XXI. *Researches on Heat.—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat.* By JAMES D. FORBES, *Esq.*, *F.R.SS. L. & E.*, *Professor of Natural Philosophy in the University of Edinburgh.*

[Continued from p. 81, and concluded.]

26. A MORE minute analysis of the influence of surface upon heat is what we now propose. And three questions present themselves for immediate solution:—(1.) If deficiency

of polish produce a variation in the proportion of not less than 3 to 1 in the quantity of transmitted radiated heat from different sources, can we employ salt plates with the ordinary degree of polish, and yet consider them as equally transparent for every kind of heat, as M. Melloni's discovery has hitherto entitled us to do? (2.) Is the effect of roughness common to other substances as well as rock-salt? (3.) The operation of depolishing with sand-paper is nothing more than the making of an infinite number of distinct grooves on a polished surface; supposing these grooves to be regularly formed, and capable of numerical estimation, will the effect continue?

27. (1.) With respect to the first of these questions, it is satisfactory to be able to answer it affirmatively in a general way. I took two salt plates, of which the surfaces had not been *regularly* polished for a long time, and which, though bright and clear, were by no means particularly even and true. Of heat from Locatelli's lamp previously sifted by glass, these *four surfaces* of rock-salt transmitted 72 per cent. With dark heat from smoked brass the per-centage was 73, a difference which, in this experiment, could hardly be considered as appreciable. *The transmission of these two very different kinds of heat was therefore equal.* M. Melloni has shown that when rock-salt is pure and perfectly polished, ·92 of the incident heat is transmitted by a pair of surfaces, and therefore four surfaces should transmit $(\cdot 92)^2$ or 84·5 per cent. This estimate I have verified, and am satisfied of its accuracy. The deviation in the present case (which I think it right not to pass over) is due partly, no doubt, to the inequalities of surface, but chiefly to some imperfections in the salt itself, which, as the experiment was merely a relative one, were not adverted to. In contrast with this, I used at the same time (December 11, 1839) a piece of salt, which once had been polished on both sides, but which, by being laid aside for some years, had become completely dull and gray on its surface. This specimen, then, was simply *depolished*; it contained no furrows, and had been subjected to no mechanical action whatever. Its per-centage of transmission was,

	Locatelli, with Glass.	Dark hot Brass.
Tarnished salt	66	77

clearly establishing the general principle.

28. (2.) With respect to the question, whether roughness of surface has a similar effect in modifying the diathermancy of other substances as well as rock-salt, we are able to give a distinctly affirmative answer. Rock-salt being, so to speak, quite indifferent to the quality and source of the incident heat,

any cause of specific action becomes immediately apparent. Not so with any other substance, which, exercising already a specific action in virtue of its nature, is to have that specific action modified by a modification of surface. At least the question is, whether or not this modification will occur? An example will best illustrate how this modification may be discovered and expressed. I took a plate of mica with its natural bright surfaces, and so thin as to transmit in considerable abundance heat from different sources. The per-centages in this state were determined as follows:—

	Locatelli, with Glass.	Locatelli.	Dark Heat.
Mica with *bright* surfaces ...	83·5	74	37

Both sides of the mica were depolished with emery-paper, and the experiment repeated (27th November, 1839):

	Locatelli, with Glass.	Locatelli.	Dark Heat.
Mica with *rough* surfaces, ...	45·5	51	31·5

Denoting the original transmissions by 100, the diminished effect due to the roughness of the surface will be represented by

54	69	85

demonstrating as clearly as possible that the stoppage is proportioned to the temperature of the source of heat; thus, whilst 46 per cent. of the first kind was arrested by the roughness of the surface, only 15 per cent. of dark heat was stopped.

29. (3.) With regard to the third question, the action of a comparatively small number of scratches on a polished surface, instead of a general diminution of its polish, I proceeded thus:—I caused a series of extremely minute lines to be drawn mechanically with a diamond point, on a well-polished surface of rock-salt, so as to divide it into squares having *one-hundredth* of an inch for their side. A similar plate was scored by fine lines in the same manner, parallel to one another, and *one two-hundredth* of an inch apart. A portion of this second plate was crossed rectangularly, by lines drawn at the same distance, so as to divide the surface into squares four times smaller than in the first instance. These three media gave the following results with two very different kinds of heat (December 6–11, 1839):—

Source of Heat.	Scored in squares 100 lines to the inch.	Scored in lines 200 to the inch.	Scored in squares 200 lines to the inch.
Locatelli, with glass	76·5	61·5	45
Dark hot brass	82·3	68·5	64·5 *

* The part of the second plate which was scored *across* being more free from flaws than that which was *once* scored, explains the little difference between this number and that in the preceding column.

For heat of 212° the per-centage was still higher, as will afterwards be shown.

30. *Metallic Gratings.*—If the mere defect of transparency were the cause of the peculiar action of scratched surfaces, we might expect that any opake filaments would act in the same way. Could we dispense with the *medium* altogether, and employ a *screen*, which should have the qualities which we had artificially given to the physical surface of the medium, we should evidently have advanced a step in the interpretation of the phænomena. The action of grooved surfaces and gratings upon light suggested so forcible an analogy, that before I was able to procure the mechanically striated surfaces, described in the last article, I had employed fine metallic wire-gauze as a diffraction-screen, hoping to obtain results similar to those which I anticipated, and afterwards did obtain, by drawing fine lines upon rock-salt.

31. The fact that diffraction-phænomena in light, produced by gratings, are wholly irrespective of the nature of these gratings, as, for instance, whether they be formed of metal-wires, or mere lines drawn through a soapy film stretched on glass, gave some countenance to this experiment. I was not unaware that diffraction spectra are produced, not by a parallel beam of light, but by a picture, formed of a distant luminous point. Still, though the ground or field illuminated by parallel rays passing through a grating must evidently have a uniform tint, it does not appear absurd to suppose that that tint may be different from white. Nor does this question appear to have occurred to mathematicians or optical writers, until the problem presented itself to me in the course of this investigation.

32. With such wire-gauze as I could easily procure, I failed in obtaining any peculiarity of action as relates to heat from different sources; and further, the quantity of heat intercepted by the metallic grating appeared to be nearly, or exactly, proportional to the surface of the opake portion of the screen. Thinking that perhaps finer gauze than that I used (60 wires to the inch) might produce the desired effect, I obtained, through the kind assistance of Sir John Robison and M. Leonor Fresnel, the finest manufactured in Paris, going as high as about 160 per inch. In general my first results were confirmed, viz. (1.) that the proportion of heat stopped is irrespective of the source; (2.) that it is to the incident heat as the area of the wires is to the area of the surface. It must be observed, however, that the determination of this latter proportion with extreme accuracy by an examination of the grating, is not so easy as might at first sight appear. When the

wire is fine compared to the *interstices*, the interstices are pretty nearly rectangular and equal-sided. But this is not the case in most manufactured wire-gauze. One set of wires is nearly parallel and straight, but not so the set interlaced with the former, which do not generally make their intersections at right angles, and hence, universally, the interstices are somewhat smaller than a calculation proceeding upon the number of wires per inch, and their diameter would give. Distrusting my own observations, I put three specimens of wire-gauze into Mr. John Adie's hands, requesting him to determine the mean diameters and intervals of the wires. With a very accurate micrometer he determined 14 values for each of these quantities in both directions. From these data the proportion of the interstices to the whole surface of each grating is easily calculated, and the results are given below for three sorts of gauze of which I had *previously* determined the permeability for heat.

Micrometric Measurement of Wire-Gauze. Unit of Measure = $\frac{1}{32400}$ inch.

Wire-Gauze.	Lengthwise.		Breadthwise.		Ratio of Interstices to Surface.
	Interstice.	Wire.	Interstice.	Wire.	
No. 1. (57 per inch)	534	371	562	384	·3504
No. 2. (92 per inch)	375·6	179·4	402·6	179·6	·4680
No. 3· (129 per inch)	200	159	284	168	·3500

33. The numbers in the last column (computed on the supposition of the interstices being geometrical rectangles) are to be compared with the following experimental determinations of the proportion of incident heat transmitted by these gratings.

Proportions per 100 of Incident Rays of Heat transmitted by Wire-Gauze.

Wire-Gauze.	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.	Hot Water.
No. 1. (57)	32·5	32	33·5	
No. 2. (92)	46·0*	...	44·7 †	
No. 3. (129) ...	30·5	...	30	29·7

* Two such gratings superimposed, so that the wires formed angles of 45° with one another, gave for the per-centage of transmission 20·7. The square root of this, or effect due to each grating, is 45·5, or almost the same as the number in the text.

† Two superimposed gratings gave 21·2 per cent., or 46 for each system separately.

The differences for each grating, perhaps, do not exceed the errors of experiment. In every case these numbers are *inferior* to the geometrical interstices, but what inclines me to think that this difference is due to the irregularities of figure of the gauze (including the effect of *flattening* of the wires, where they overlap, making the interstices obtuse-angled) is this: that No. 2, in which the wires were finer compared to the interstices than in others (the total interstices being one-third part larger in proportion), and the gauze evidently far more regularly formed than in the other cases, the per-centage transmitted differs very little from the geometrical gauge. I own, at the same time, that a difference of 5 per cent. in No. 3 (which is evidently not due to an error of observation), seems to me barely accounted for by this remark.

34. *Thread-Gratings.*—With gratings of fine cotton-threads $\frac{1}{100}$dth inch apart, used for showing Fraunhofer's Spectra, I obtained a similar result. These threads were arranged parallel-wise on two frames, capable of being superimposed rectangularly. Thus, we can either employ a screen of parallel threads *one-hundredth* of an inch apart, or a screen of mathematically accurate squares, formed by superposition. It is difficult in this case, however, to obtain the diameter of the thread accurately enough to estimate the ratio of interstices.

Per-centage of Incident Heat transmitted by Cotton-Thread Gratings, $\frac{1}{100}$dth inch apart.

	Locatelli, with Glass.	Dark Heat.
Thread Grating, SINGLE	29·5	30·2
——— ——— DOUBLE	9·0 *	8·3 †

* Corresponding *single* action = 30·0 per cent.
† ——————— = 28·8 ———

The difference here seems imperceptible, the differences, such as they are, being in opposite directions. The results in the last column are from single experiments (November 28th, 1839).

35. *Action of Powders.*—Adhering to the idea (12.) that the action of a smoked surface was due to the mechanical action of a number of minute opake points distributed over a transparent body, it occurred to me almost at the commencement of these experiments, to try the effect of powders artificially sifted on such a surface. Any ingredient, however, which could make the powders adhere to the surface, would hae vitiated the experiment, by introducing its own

proper diathermancy. I therefore included the powders between two polished plates of rock-salt, closed at the edges with wax. The preliminary experiment (27.), to show that the salt surfaces, in the state in which I commonly employed them, exercise no perceptible influence on the quality of the transmitted heat, was evidently a very important one for the conclusions I meant to draw. It was, as I have stated, quite satisfactory.

36. The first experiments which I made with powders (December 6, 1839), were with chalk and alum, finely dusted between two plates of salt. I selected the chalk on account of its absolutely uncrystalline and opake character; and alum, because its power of stopping rays of heat of low temperature was so very great, that I judged that if the influence as a *mechanical modifier of surface* should prove predominant, and allow as much, or more, heat of *low* than of *high* temperature to pass, the *mechanical* influence of a substance in fine powder would be clearly established.

37. Now, the result at which I arrived, and which was entirely conformable to my anticipation, may serve to show the caution requisite in drawing conclusions from limited data, however apparently conclusive. The surfaces powdered with chalk suffered *rather more* heat of low than of high temperature to pass (viz. 34·5 per cent. dark heat, and only 30·5 of heat from Locatelli lamp, transmitted through a thick glass-lens), whilst the salt strewed with alum appeared quite indifferent to the kind of heat incident* (transmitting only 17 per cent. of both, thus showing that the powder was in considerable quantity). I concluded, therefore, with apparent reason, that the chalk having no *specific* action, or being (most probably) opake or *athermanous*, the powder of it acting mechanically, allowed low temperature heat to pass in excess, whilst in the case of alum, the *specific* action was entirely counteracted by the *mechanical* action of the powder. I simply stated the fact amongst others detailed in the preceding pages, in a memorandum presented to the Royal Society of Edinburgh on the 16th December 1339†, and a few days after, in a slightly different form, communicated to M. Arago, and printed in the *Comptes Rendus de l'Académie des Sciences*, 6th January 1840. On the 28th of December, I obtained a similar result for charcoal powder (whose affinity with smoke

* Yet an alum plate of a certain thickness transmits no less than 27 per cent. of the one kind of heat, and *no sensible portion* of the other (Melloni.)

† See our July Number, p. 69. note *.

suggested its use), and yet it does not appear that the general conclusion which I intended is entirely warranted.

37. It is well known that Sir Isaac Newton overlooked the variable dispersive power of bodies for light, in consequence of having compared two, in which the dispersion *happened* to be proportional to the mean refraction. A similar haste to generalize would have led to error on the present occasion, had not a simultaneous investigation led me to re-consider the subject of powders. Whilst waiting for the arrival of fine wire-gauze from Paris, it occurred to me to try the effect of metals in a state of extreme division. It seemed, however, first desirable to ascertain whether the metals are as incapable of transmitting heat as is commonly supposed.

38. For this purpose, I stretched a piece of the thinnest gold-leaf across a wide diaphragm of pasteboard, and suffered an intense parallel beam of heat from Locatelli's lamp to fall directly upon the pile. A screen of glass was interposed, which, by experiment, was known to stop 43 per cent. of this sort of heat. The needle of the galvanometer deviated 31°·2, the glass being interposed; the equivalent direct effect would have been $31{\cdot}2 \times \frac{100}{43} = 72°$. When the glass was removed and the gold-leaf put in its place, on the brass screen being alternately introduced and removed, not the faintest motion was perceptible in the needle; had it amounted to $\frac{1}{20}$th of a degree, that is, had $\frac{1}{1400}$dth of the incident heat been transmitted by the gold-leaf, I considered that the effect would have been perceptible. Yet this gold-leaf was so thin that the features of a landscape could be distinctly seen through it, of the usual bluish-green tint. No more convincing proof certainly can be desired, that conduction plays no sensible part in these experiments, since it did not sensibly act on a film of one of the best-known conductors of heat, and perhaps not more than $\frac{1}{300000}$dth of an inch thick. I thought it worth while to repeat the experiment with dark heat, and with the same results. The analogy of the action of split mica on light to metallic reflexion led me to suspect, that if any kind of heat were transmitted by metallic leaves, it would be that of low temperature.

39. The imperviousness to heat of gold-leaf, the thinnest continuous film of metal which we can obtain, satisfied me of the importance of obtaining the metals in a condition to verify my experiments with the powder of other substances. When the hope diminished of obtaining wire-gauze of a degree of fineness (I mean fineness in the *wire*, not closeness of texture, for that was comparatively immaterial) which might

vie with the diamond scratches on the salt surface, which presented, under the microscope, an irregular furrow, probably nearly $\frac{1}{2000}$dth of an inch in mean breadth,—I recurred to the project of using the metals in *powder*. It was evident, from the experiments on depolished and scored surfaces, that the *irregularity* of these streaks had nothing whatever to do with the phænomenon of checking rays of high refrangibility and admitting others. Sand-paper scratches, than which nothing can be more irregular, produced the effect, and that more intensely as the surface became more coarsely and closely furrowed. Nay, it occurs in natural tarnish, where there can be no linear arrangement of the points affected. It seemed to me, therefore, that a surface covered with a metallic powder, presented the *limit* of a grating where the interstices were not required to have any regular form.

40. The next difficulty was to obtain impalpable powders indubitably metallic, to which I attached very considerable importance, for it was quite conceivable that the metallic sulphurets and other substances employed for the fictitious metallic powders called gold, silver, and copper bronzes, might have specific diathermancies which might injure the experiment. I at length succeeded in obtaining silver by precipitation, and copper from Daniell's battery; and with some difficulty I procured from a large manufacturer coinage silver and gold, reduced by mechanical trituration to a perfectly impalpable and beautifully metallic powder. These expensive preparations are now wholly superseded by the admirable fictitious bronzes in use in the arts. These, together with metallic copper-bronze, perfectly impalpable, furnished by the same individual, and a much coarser tin powder used by druggists, formed the material of a very careful series of experiments, which I extended over a very considerable period, and varied in a great many ways. (1840, Jan. 28, &c.)

41. The following table contains the results of my experiments on metallic powders, which (with the exception of tin) may be considered as perfectly impalpable, adhering to the dry finger, and *undoubtedly metallic*.

Per-

Per-centage of Heat from different Sources transmitted by Metallic Powders.

Powder.	Locatelli Lamp.			Dark Hot Brass.	Hot Water.	Remarks.
	Glass interposed.	Direct.	Smoked Salt interposed.			
Gold, No. 1. (adhering to a single surface of salt)	58	...	50·5			
Gold, No. 2. (between two salt plates)	7·4*	...	...	4·1*		
Silver, No. 1. (between two salt plates)	25·3	24·2	...	21·8	...	Mean of a considerable number of results.
Silver, No. 2. (adhering to a single surface of salt)						The same plate was used, but differently placed in respect of the pile, so that each series stands by itself.
1st Series	27·7	...	18·5	...	...	
2nd Series	29·5	...	22·1	...	25	
Copper, No. 1. (between two salt plates)						
1st Series......	14·8	...	16·0			
2nd Series	17·4	...	...	18·7	17	
Copper, No. 2. (between two salt plates)	5·6*	...	...	4·05*		
Tin (between two salt plates)	27·0	26·0	...	25·5	...	Mean of a considerable number of results.

42. These observations are confessedly very imperfect. I am persuaded, however, that the apparent anomalies are not errors of observation; other instances will presently occur. With a view to determine the quality of thickly strewed surfaces yielding a very feeble per-centage of transmitted heat, it was desirable to use an intense incident beam. In order, however, to keep the comparison within the range of galvanometer degrees, whose numerical values have been tested (Second Series, arts. 7–8), the observations in the preceding table marked thus * were made in the following manner. The *direct* effect of the incident heat on the pile was never observed, but only that part of it which penetrated the wire-gauze, No. 3 of art. 33, which transmits almost exactly 30 per cent. of every kind of heat. The direct effect was estimated at $\frac{100}{30}$ of the degrees of deviation corresponding to this transmission, and then the wire-gauze being removed, and the medium to be examined substituted, the effect was compared to the computed direct effect. For example, with the copper powder, No. 2, the effect of the Lo-

* See the next article (42.)

catelli lamp, heat transmitted through thick plate-glass, and then modified by wire-gauze, was .... 22°·57

$$\text{Direct effect} = 22{\cdot}57 \times \frac{10}{3} \quad \ldots\ldots\ldots \quad 75{\cdot}2$$

Wire-gauze removed, and copper substituted ...... 4·15

Ratio to direct 5·52 : 100.

In this way per-centages may be obtained with very nice accuracy: another experiment gave in the same case 5·60 : 100.

43. The table in art. 41. demonstrates to my conviction (strengthened by a careful examination of the very consistent observations on which it is founded), (1.) That gold, silver, and tin powders, instead of having the property which I was disposed to assign to opake powders generally, do really transmit more heat of high than of low temperature; that is, act like glass, alum, and other transparent media in their common state. (2.) With respect to copper, two series give one result, and a third the opposite. Yet all of these were made with great care, and contain internal evidence of their accuracy. I am confident that the differences are not due to errors of observation; and I have observed other cases, in which an increase of thickness of the obstructing medium, and an increased intensity of the incident heat, gave altered results as to permeability, a result by no means paradoxical, since intense heat may be sensibly transmitted through a nearly opake substance, and thence acquire a new character, which a feebler beam, transmitted through a less obstructing medium, would not possess. At all events, I can offer no further explanation at present. That copper possesses a peculiar character, distinct from the other metals which I tried, I am fully persuaded.

44. The evidence which the experiments on metallic powders gave of the inadequacy of the *mere powdery form* to produce the effect of smoke, forced me to a more critical examination of other bodies in a similar state.

45. I repeated my experiments with increased care on the powders already employed. I tried a great number of new ones, chosen amongst substances differing as widely in nature as possible. Some of these substances were repeatedly tried in different specimens, the powder more or less thickly strewed, and at different times.

46. One circumstance in particular raised a doubt as to the result of my former conclusion, where it seemed most incontrovertible. I had argued, that if alum in powder arrested equally all kinds of heat, the *mechanical* action of the powder must have opposed and destroyed the *specific* action of the alum (36.). I was gradually, however, led to admit, that, *in the state of powder, most diathermanous bodies are almost*

equally opake, or, rather perhaps, I should say, equally indifferent to the *kind* of incident heat (*i. e.* colourless in optics).

47. So far as the eye could judge of the proportion of obstacles in surfaces strewed with different kinds of powders, there did not seem any very marked peculiarity in their transparency for heat. A surface dusted with alum or citric acid appeared to transmit nearly as much as one strewed with powdered rock-salt. Nor could this arise merely from the *minute thickness* of the substance, which is well known to produce in heat, as in light, an approximation to a colourless character; for the proportion stopped by the powder was always a large fraction (usually from $\frac{3}{4}$ths to $\frac{9}{10}$ths) of the incident heat. The opacity, then, is the result of the innumerable reflexions and interferences which *scatter* and *stifle* the transmitted heat; and this is almost equally effectually done, whatever be the nature of the substance. On reflection, therefore, this general result does not appear surprising. I will quote one experiment, in particular, in illustration of it.

48. When I was at a loss to procure fine metallic fibres, I thought of employing a diaphragm irregularly covered with fine threads of spun glass, with a view (just as in the case of the alum powder) of ascertaining how far the mechanical condition of the glass might modify its well-known qualities with respect to the transmission of heat. When Locatelli lamp-heat, having been transmitted by *thick plate-glass*, fell upon the spun-glass fibres, forming an irregularly reticulated diaphragm, no more than 47·5 per cent. of the incident heat was transmitted. Now, we know perfectly from the experiments of De la Roche and Melloni, that, after passing through such a thickness of plate glass, an additional film, the thickness of the glass fibres used would produce *no sensible resistance* to the further passage of the heat, excepting only its superficial reflexion. The loss of 52·5 per cent. of the heat was therefore due to the *scattering* and *stifling* of heat by reflexion at the surfaces of the fibres, refraction through their cylindric surfaces, and interference. We cannot, therefore, be surprised, if the refracted part of the heat reaching the pile (the only portion very materially affected by the nature of the medium) should not greatly alter the quantity of different sorts of heat indicated by the galvanometer. Accordingly, we find, that heat from a dark surface of brass warmed by an alcohol lamp, had 44 per cent. transmitted under the same circumstances; and even hot water had 42 per cent., although a small thickness of glass is sensibly opake for that kind of heat.

49. If this be the case,—if the differences be so trifling—for a reticulation of regularly-formed, transparent, and polished threads of glass, much more must it hold with impalpable crystalline or other powders, presenting (no doubt) mi-

nute surfaces at every angle, and minute fissures in every direction.

50. The following table contains the results of a large number of experiments on powders of various kinds, many of them repeated under various circumstances. The investigation is, as in the case of the metallic powders, confessedly imperfect; but since the broad simple principle which I at first tried to establish respecting the diathermanous quality of opake powders does not appear to hold universally, I stopped this series of experiments, which were troublesome and laborious, after establishing a few general facts, which I will presently lay down, without attempting to exhaust a subject, of which, by and by, we shall no doubt know more, but which at present it would be perhaps a waste of time to pursue into its insulated details. These powders were in all cases dusted between polished salt-plates, united at the edges, and then attached to diaphragms of card, so arranged as to transmit the heat in every case through the same parts of the surface.

Per-centage of Transmission of Heat, from different sources, through Non-metallic* Powders.

Powder of	Source of Heat.			
	Locatelli Lamp		Dark Hot Brass.	Hot Water.
	Through Glass.	Through Smoked Salt.		
Alum, No. 1.	17·0	...	17·1	...
—— No. 2.	15·2*	...	13·0*	...
Citric acid, No. 1	29	30	33 †	31·5
—— No. 2.	12·9*	...	8·7*	...
Rock-salt, No. 1.	12·8* 13·4	11·8	11·3	...
—— No. 2.	31·5 ‡	...	29·2‡	...
Sulphur	50·0	...	44·7	...
Red-lead	30·2	...	34·0	...
Galena	26·3	22·4	...	...
Charcoal A.	5‖	...	9 ‖	...
—— No. 1. 1st Series §	11·4	13·9	...	...
—— 2nd Series §	15·1	...	16·0	17
—— No. 2.	3·2*	...	3·5*	...
Chalk, No. 1	30·5	...	34·5	...
—— No. 2.	15·5* 15·6	18·4	17·9	...
—— No. 3.	27·5	...	32·0	...
Carbonate of magnesia	8·3	12·6	...	...

* The observations marked thus were made with a powerful beam of heat in the way described in art. 42. † Not directly comparable with the other two observations on the same line, and probably 3 or 4 per cent. too high. ‡ Extremely good observations.

‖ The intensities very feeble. § The circumstances in these two series varied, so as to make the one not directly comparable with the other; but each is perfectly good.

* By *non-metallic* is meant, not in the state of a *pure* or uncombined metal.

51. On the preceding table, I would observe, (1.) That the pulverized crystalline bodies, such as rock-salt, alum, citric acid, and sulphur, exhibit no decided tendency to transmit an excess of heat of low temperature, depending on their powdery form. The carefully repeated experiment with rock-salt is, on this point, very conclusive, since its indifference as a substance to the quality of the heat which it transmits would at once leave the effect, if any, due to mechanical condition, apparent. It even very evidently appears in this state to transmit *less* freely heat of low than heat of high temperature. (2.) Galena, the crystallized sulphuret of lead, in fine powder, appears to possess the qualities of gold, silver, and tin (43.). (3.) Red lead, charcoal, chalk and magnesia, all substances in an opake *earthy* condition, appear certainly to transmit an excess of *dark heat.* I think it probable that this list might be extended to most bodies having a similar mechanical constitution.

53. These distinctions, I am well aware, leave the *causes* of the difference of character of powders, and the peculiarities of tarnished surfaces, nearly in the same obscurity as before. In particular, I cannot but regard it as being singular, that a surface covered with powdered salt has no analogy, but even opposite properties, to one of the same material mechanically furrowed*. The contrariety of action of metallic powders to those of opake earths, is as singular as it was unexpected. I have already stated, however, my doubt whether a complete investigation of the peculiarities of specific substances would, *at present*, reward the necessary labour. I have made experiments on a few fibrous substances, as paper and membrane, which I thought might very probably act as tarnished surfaces do. There is an approximation to this, as will be seen, in the common cambric or tissue paper. In the kind of tracing paper employed (which is made in Paris, I believe, under the name of *papier végétal*), there is evidently some foreign matter introduced to produce the transparency, which modifies the transmission. A close reticulation of cotton fibres has already been shown to exercise no specific action (34.). The following table contains a few results not included in preceding ones, and illustrating in several substances the quality of *heat-colour*, which in this paper we have been considering.

* To put this in the most clear point of view, I used and compared two such plates in the same experiment.

Per-centage of Heat transmitted by several Bodies.

Substance.	Source of Heat.		
	Locatelli Lamp, with Glass.	Dark Hot Brass.	Hot Water.
Gold-beater's skin	60	28	...
Cambric or tissue paper	8·6	10·5	...
Tracing paper (*Papier végétal*)	36	28	...
Fibres of spun glass............	47·5	44	42
Smoked salt.....................	30·2	58	67
Roughened salt	49	73	76
Polished salt, scored into 200 × 200 squares per inch	49·5	73	77

54. The leading facts contained in this paper are these :—

55. I. The peculiar (*red-like*) character of films of smoke in transmitting heat of low temperature is partaken,—

A. By simple powder of charcoal.

B. By (at least some) other dull earthy powders.

C. By surfaces simply *dull* or devoid of polish.

D. By surfaces irregularly furrowed, as with emery or sand-paper.

E. By polished surfaces, on which fine distinct lines have been drawn.

F. By the mechanical lamination of transparent mica, which, as a continuous medium, possesses opposite properties.

56. II. The following media seem *indifferent* to the kind of heat which they transmit :—

A. The thinnest gold-leaf is impervious to any.

B. Metallic gratings transmit all kinds of heat in a proportion which is probably exactly as the area of the interstices which they present.

C. Thread-gratings.

D. In a state of powder, most crystalline bodies *approach* to a condition of opacity for heat.

57. III. The following bodies, in addition to those commonly known, transmit most heat of high temperature (*violet-like heat*).

A. Several pure metallic powders.

B. Rock-salt in powder; and many other powders.

C. Animal membrane.

58. IV. Heat of low temperature is most regularly reflected at imperfectly polished surfaces. It is also, we have seen, most regularly transmitted. These facts are of great importance to the theory of heat; and may probably sug-

gest inquiries of no small interest with regard to light, and especially the phænomena of absorption.

59. We have already (24.) noticed the analogy which the fact stated in the last article bears to the easier reflexion of red than violet light from depolished surfaces, and in that fact we find a confirmation of the application of the undulatory doctrine to heat, and of the opinion that the waves producing heat are longer in proportion as the temperature of the source is less. The phænomena of transmission are more obscure; they may be compared either to the diffraction, or to the absorption, of light.

60. The action of lines on polished surfaces, similar to those used in many diffraction experiments, led to the inquiry (31.) whether the mean colour of light transmitted by gratings was necessarily unchanged? The question does not seem to have occurred to any one to whom I have mentioned it; and though the most likely result would seem to be, that there should be no change, the grounds of such an *à priori* opinion do not appear absolutely conclusive. Professor Kelland, however, has, I believe, first succeeded in integrating the expression for the illumination of a screen placed behind a grating of any kind (see Airy's Mathematical Tracts, page 328) on which a plane wave falls, and he informs me, that in every case where the breadth of the interstices is any multiple of the breadth of the wires or opake spaces, the intensity is the same as if there were a diaphragm equal in size to the sum of the interstices of the grating.

61. This result (which seems quite sufficiently general for our purpose) is so far confirmed by the absolute *indifference* of metallic gratings to the quality of the incident heat.

62. It remains, however, to be explained how furrowed surfaces can act, except by intercepting, as an opake network would do, a part of the heat. I cannot give an explanation which appears full and satisfactory, but the condition of mica split into thin laminæ by heat, and producing the same effect, may serve to guide us, perhaps, to something like the true cause.

63. A number of thin plates, of *exactly uniform* thickness, would transmit a certain colour, and reflect the complementary one. If there be a great preponderance of plates approximating to a certain thickness, and if the disproportion of the lengths of the incident waves be great, a large proportion will be in like manner transmitted, and the remainder stifled or reflected. If this effect is not so frequently observed in bodies mechanically separated into films as we might expect, this is owing to the small range of length of wave in the visible parts

of the spectrum. A small variation in the thickness of the film transmits or annihilates by interference each colour of the spectrum in succession. If the waves of heat be much more heterogeneous (as I have already surmised) than those of light, such effects would be proportionably more sensible.

64. Possibly a grooved surface may be considered as presenting a number of polished surfaces, partially detached from the general surface, under small obliquities to the incident rays; and we may suppose that these rays, after separation by partial reflexion and refraction, reunite with unequal retardations, producing *first* a destructive effect upon the shorter waves, and suffering the others to persevere. I have already adverted to the fact, that most turbid fluids transmit chiefly the longer luminous waves. I offer these, however, but as vague conjectures upon a very obscure subject. I think that experiments on the colour of media, such as those we have employed, and especially of depolished plates, might not be without value in illustrating the phænomena of absorption in optics.

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65. In conclusion, it might perhaps be expected that I should take some notice of the experiments and reasonings of which M. Melloni has addressed an account to M. Arago, in two letters dated the 4th and 14th of March last, and published in the *Comptes Rendus* for the 30th of the same month. These letters were occasioned by the announcement of my Researches, in the same work, for the 6th of January. The present paper, founded *solely* upon experiments undertaken and completed before the despatch of the earliest of M. Melloni's communications, will, I think, sufficiently answer all the questions which are started in his letters to M. Arago, at least all those in which my experiments are concerned.

May 12, 1840.

XXII. *On a new Theorem in the Calculus of Finite Differences, with its Application to the Development of the Cosine of a Multiple Arc in descending Powers of the Cosine of the Simple Arc.* By JAMES BOOTH, *M.A., Principal of, and Professor of Mathematics in Bristol College*.*

THERE is perhaps no problem in pure mathematics which has given rise to more discussion than the development of the cosine of a multiple arc in descending powers of the cosine of the simple arc. While Euler and Lagrange, on the one hand, have evolved coincident results by methods very

* Communicated by the Author.

different, so Poinsot, on the other, having shown that the methods pursued were fallacious, and the results themselves false, has given the true development by a method complex in the extreme, founded on the principles of a higher analysis than is generally introduced into such investigations.

It appears somewhat strange, that even since the correct series has been given, the erroneous developments should have been retained in some recent publications on the subject.

In the following pages the method pursued will be found different from those generally adopted in the investigation, no principles being assumed beyond such as may be deduced from elementary algebra; while the method of determining the coefficients of the expansion, by the aid of the fundamental theorem first established, will, it is hoped, be considered simple.

Let $a, b, c, d \dots l$ be any m numbers, positive or negative, integral or fractional, or even imaginary, m being less than the integer n, and h being any finite number whatsoever, then we shall have the following theorem:—

$$\left.\begin{aligned} 0 = \{a,b,c,d \dots l\} - n\,\{(a-h)\,(b-h)\,(c-h)\dots\} \\ + \frac{n \,.\, n-1}{1\,2}\,\{(a-2\,h)\,(b-2\,h)\,(c-2\,h)\dots\} - \frac{n\, n-1 \,.\, n-2}{1\,2\,3} \\ \{(a-3\,h)\,(b-3\,h)\,(c-3\,h)\dots\}\ \&c.\ \&c. \end{aligned}\right\}(\mathrm{I.})$$

or, separating the symbols of operation from those of quantity, we may write the theorem thus:—

$$0 = (1-1)^n\ \mathrm{I}_0^n\,[(a-i\,h)\,(b-i\,h)\,(c-i\,h)\dots(l-i\,h)],$$

the symbol I_0^n denoting the sum of all the values which the quantity within the brackets takes, when i is made to assume every integral value from 0 to n inclusive.

This theorem may be proved as follows: Let the sum of the series (I.) be $=$ V, assume the arbitrary quantity k a multiple of h, and let $\alpha, \beta, \gamma, \delta \dots \lambda$ be the differences of the m numbers $a, b, c, d \dots l$ and k, so that $a = k - \alpha$, $b = k - \beta$, $c = k - \gamma$, $d = k - \delta, \dots l = k - \lambda$, then we shall have the equivalent expressions

$$\{a, b, c, d \dots l\} = k^m + \mathrm{A}\,k^{m-1} + \mathrm{B}\,k^{m-2} \dots\dots \mathrm{M}\,k + \mathrm{N}.$$

In the same way, if $k' = k - h$, we shall have

$$(a-h)\,(b-h)\,(c-h)\dots = k'^m + \mathrm{A}\,k'^{m-1} + \mathrm{B}\,k'^{m-2} \dots \mathrm{M}\,k' + \mathrm{N}.$$

Again, let $k'' = k - 2\,h$, and we find

$$(a-2h)\,(b-2h)\,(c-2h)\dots\dots = k''^m + \mathrm{A}\,k''^{m-1} + \mathrm{B}\,k''^{m-2} \dots\dots \mathrm{M}\,k'' + \mathrm{N}.$$

It is manifest that the coefficients A, B, C,... M, N are the same in the successive developments, as being functions solely of $\alpha, \beta, \gamma, \delta \ldots \lambda$ and m; making these substitutions in (I.), this series may be now thus written :—

$$\begin{aligned}
V = & \{k^m + A k^{m-1} + B k^{m-2} \ldots\ldots M k + N\} \\
& - n \{k'^m + A k'^{m-1} + B k'^{m-2} \ldots\ldots M k' + N\} \\
& + \frac{n \,.\, n-1}{1\,2} \{k''^m + A k''^{m-1} + B k''^{m-2} \ldots\ldots M k'' + N\} \\
& - \frac{n \,.\, n-1 \,.\, n-2}{1\,2\,3} \{k'''^m \text{ \&c. \&c.}\}.
\end{aligned}$$

Now, putting for k', k'', k''', their values $(k-h)$ $(k-2h)$, &c., or, as k is an arbitrary multiple of h, let $k = u\,h$, and adding the terms of the above series vertically, we find

$$\begin{aligned}
V = h^m & \left\{ u^m - n(u-1)^m + \frac{n \,.\, n-1}{1\,2}(u-2)^m \right. \\
& \left. - \frac{n \,.\, n-1 \,.\, n-2}{1\,2\,3}(u-3)^m \ldots, \text{\&c.} \right\} \\
+ A\,h^{m-1} & \left\{ u^{m-1} - n(u-1)^{m-1} + \frac{n \,.\, n-1}{1\,2}(u-2)^{m-1} \right. \\
& \left. - \frac{n \,.\, n-1 \,.\, n-2}{1\,2\,3}(u-3)^{m-1} \ldots \text{\&c.} \right\} \\
+ B\,h^{m-2} & \left\{ u^{m-2} - n(u-1)^{m-2} + \frac{n \,.\, n-1}{1\,2}(u-2)^{m-2} \right. \\
& \left. - \frac{n \,.\, n-1 \,.\, n-2}{1\,2\,3}(u-3)^{m-2} \ldots \text{\&c.} \right\} \\
& \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\
& + M\,h \left\{ u - n(u-1) + \frac{n \,.\, n-1}{1\,2}(u-2) \ldots \text{\&c.} \right\} \\
& + N\,h^0 \left\{ u^0 - n(u-1)^0 + \frac{n \,.\, n-1}{1\,2}(u-2)^0 \ldots \text{\&c.} \right\}.
\end{aligned}$$

Now, by a known theorem in the calculus of differences, the quantities between the brackets are the nth differences of the mth, $(m-1)$ and lower powers of the number $(u-n)$. Hence

$$\begin{aligned}
V = hm\,\Delta^n (u-n)^m + A\,h^{m-1}\,\Delta^n (u-n)^{m-1} \ldots\ldots\ldots \\
+ M\,h\,\Delta^n (u-n) + N\,h^0\,\Delta^n (u-n)^0;
\end{aligned}$$

and as m by hypothesis is less than n, the nth differences of $(u-n)m$, $(u-n)^{m-1}$ are each equal to zero ; hence, as A, B, C,

M, N, and h are finite, all the terms of the above series are separately equal to zero, or

$$V = 0.$$

It is clear that, whatever be the nature of the numbers $a, b, c, \ldots l$ and h, we may assume the arbitrary number k, so that u may be an integer.

The theorem $\Delta^n (u - n)^m = 0$ may be reduced to another more simple, as follows: let $u = x + n$, then $\Delta^n (u - n)^m = \Delta^n x^m$; but

$$\left.\begin{aligned}\Delta^n x^m = (x+n)^m - n(x+n-1)^m + \frac{n \cdot n-1}{1\ 2}(x+n-2)^m \\ - \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}, \text{\&c.} \quad \ldots\ldots\end{aligned}\right\}(\text{II})$$

(*Lacroix.* tom. iii. p. 9.)

developing these powers in the following manner:—

$$(x + n)^m = n^m + P n^{m-1} + Q n^{m-2} \ldots\ldots S n + T$$

$$(x + n - 1)^m = (n-1)^m + P(n-1)^{m-1}$$
$$+ Q(n-1)^{m-2} \ldots\ldots S(n-1) + T.$$

....

P, Q, S, T being functions of x and m. Substituting these values of $(x+n)^m$, $(x+n-1)^m$, &c. in (II.), we find

$$\Delta^n x^m = n^m + P n^{m-1} + Q n^{m-2} \ldots S n + T$$
$$- n \{(n-1)^m + P(n-1)^{m-1} + Q(n-1)^{m-2} \ldots . S(n-1) + T$$
$$+ \frac{n \cdot n-1}{1\ 2} \Big\{ (n-2)^m + P(n-2)^{m-1}$$
$$+ Q(n-2)^{m-2} \ldots S(n-2) + T, \text{ \&c. \&c. \&c.}$$

Now, adding vertically, we obtain

$$\Delta^n x^m = \Big\{ n^m - n(n-1)^m + \frac{n \cdot n-1}{1\ 2}(n-2)^m$$
$$- \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}(n-3)^m \ldots, \text{ \&c.} \Big\}$$
$$+ P \Big\{ n^{m-1} - n(n-1)^{m-1} + \frac{n \cdot n-1}{1\ 2}(n-2)^{m-1}$$
$$- \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}(n-3)^{m-1} \ldots, \text{ \&c.} \Big\}$$

$$+ Q \left\{ n^{m-2} - n(n-1)^{m-2} + \frac{n \cdot n-1}{1\ 2}(n-2)^{m-2} - \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}(n-3)^{m-2} \ldots, \&c. \right\}$$

....

$$+ S \left\{ n^1 - n(n-1)^1 + \frac{n \cdot n-1}{1\ 2}(n-2)^1 - \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}(n-3)^1 \ldots, \&c. \right\}$$

$$+ T \left\{ n^0 - n(n-1)^0 + \frac{n \cdot n-1}{1\ 2}(n-2)^0 - \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}(n-3)^0 \ldots\ldots \right\}.$$

The quantities between the brackets in the common notation are thus written: $\Delta^n 0^m$, $\Delta^n 0^{m-1}$, $\Delta^n 0^{m-2}$, which are each cypher when n is greater than m; hence $\Delta^n x^m = 0$ when $n > m$.

The theorem $\Delta^n 0^m = 0$, or its development,

$$\left. \begin{array}{l} n^m - n(n-1)^m + \frac{n \cdot n-1}{1\ 2}(n-2)^m \\ - \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3}(n-3)^m = 0 \quad . \end{array} \right\} \quad \text{(III.)}$$

may be proved by the common principles of algebra as follows:

Assume $x = z - 1$, raising both sides of this equivalence to the nth power, we have

$$x^n = z^n - n z^{n-1} + \frac{n \cdot n-1}{1\ 2} z^{n-2} - \frac{n \cdot n-1 \cdot n-2}{1\ 2\ 3} z^{n-3}, \&c.$$

Let the first derivative of this equivalence be taken, or, in other words, let the equivalence $x = z - 1$ be raised to the $(n-1)$ power, and then multiplied by n, we thus find

$$n x^{n-1} = n z^{n-1} - n(n-1) z^{n-2} + \frac{n \cdot n-1 \cdot n-2}{1\ 2} z^{n-3}, \&c.$$

Multiplying this series by the equivalence $x + 1 = z$, we obtain

$$n x^n + n x^{n-1} = n z^n - n(n-1) z^{n-1} + \frac{n \cdot n-1}{1\ 2}(n-2) z^{n-2}, \&c.$$

Again, taking the first derivative of this series, and multiplying by $x + 1 = z$, there results the equivalence

$$n^2 x^n + n(2n-1)x^{n-1} + n.n-1.x^{n-2} = n^2 z^n - n(n-1)^2 z^{n-1} + \frac{n.n-1}{1\ 2}(n-2)^2 z^{n-2}.$$

Performing the same operations m times successively, we find

$$\left.\begin{aligned} &A x^n + n B x^{n-1} + n.n-1\, C x^{n-2} \ldots + n(n-1)\ldots(n-m+2) \\ &\quad P x^{n-m+1} + n(n-1)(n-2)\ldots\ldots(n-m+1)\, x^{n-m} \\ &= n^m z^n - n(n-1)^m z^{n-1} + \frac{n.n-1}{1\ 2}(n-2)^m z^{n-2} \\ &\quad - \frac{n.n-1.n-2}{1\ 2\ 3}(n-3)^m z^{n-3} \quad \ldots \end{aligned}\right\} \text{(IV}$$

A B C...P being functions of n.

Now let $z = 1$, or $x = 0$, and the first member of the last equation becomes a series of positive powers of 0, multiplied by constant finite coefficients, and therefore each term of the first member is equal to zero. Hence the second member becomes also zero, or

$$0 = n^m - n(n-1)^m + \frac{n.n-1}{1\ 2}(n-2)^m, \text{ \&c.}$$

Let $m = n$, then all the terms of the first member of equation (IV.) vanish, as before, except the last, and it becomes $n(n-1)(n-2)\ldots2.1.0^0$, but $0^0 = 1$. Hence

$$1\ 2\ 3\ (n-1)\, n = n^n - n(n-1)^n + \frac{n.n-1}{1\ 2}(n-2)^n.$$

From these principles it follows that if n be an improper fraction greater than the integer m, that the series (III.) continued to infinity is equal to zero.

In the general theorem (I.) let $a = n-1$, $b = n-2$, $c = n-3\ldots\ldots l = (n-t+1)$, $h = 1$, and $n = t$, then

$$\left.\begin{aligned} &(n-1)(n-2)\ldots(n-t+1) - t\{(n-2)(n-3)\ldots(n-t)\} \\ &\quad + \frac{t(t-1)}{1\ 2}\{(n-3)(n-4)\ldots(n-t-1)\} -, \text{\&c.} = 0 \end{aligned}\right\} \text{(V.)}$$

This theorem will be found useful further on.

To develope $x^n + x^{-n}$ *in descending powers of* $(x + x^{-1})$.

Put $x + x^{-1} = z$, and assume the series

$$x^n + x^{-n} = A z^n + B z^{n-2} + C z^{n-4} + D z^{n-6} \ldots \text{\&c. \&c.} \quad \text{(VI.)}$$

The following considerations will show that the assumption of the development in this form is correct; for, in the first place, if n is an odd number, and x is changed into $-x$, z will be changed into $-z$; and the first member of (VI.) re-

tains its value, changing its sign. Hence all the powers of z in the second member of (VI.) are odd, or the index n is diminished by even numbers.

Again, when n is an even number, and x is changed into $-x$, z is changed into $-z$; and the first member of (VI.) remaining unaltered, the second can contain only even powers of z, or n is diminished by even numbers.

The development of $x^n + x^{-n}$, in powers of $\left(x+x^{-1}\right)$ or z, can contain no negative powers of z.

For if it were possible that the development should contain such powers, put $x = \sqrt{-1}$, then $z = 0$, and the first member of (VI.) becomes $+ 2$ when n is divisible by 4, $- 2$ when n is divisible by 2, and not by 4, and zero when n is odd, while the second member becomes infinite, as having positive powers of zero in the denominator.

Hence it follows that when n is even, the coefficient of z^0 is ± 2; for by putting $z = 0$, all the terms of the development vanish, except the one containing z^0, while the first member of the equation (VI.) becomes ± 2.

When n is a fraction $= \frac{p}{q}$, the development (VI.) *becomes impossible.* To show this, put $x^{\frac{1}{q}} = y$, $x^{-\frac{1}{q}} = y^{-1}$; then putting $y + y^{-1} = u$, $x^{\frac{p}{q}} + x^{-\frac{p}{q}} = y^p + y^{-p}$

$$= \mathrm{P}u^p + \mathrm{Q}u^{p-2} + \mathrm{R}u^{p-4}, \text{ \&c.}$$

by (VI.), since p is a positive integer number, and P, Q, R are functions of p, as will be shown further on.

We must now substitute for the powers of u, in the last development, their values in terms of z,

$$\text{now } z = y^q + y^{-q}.$$

Hence

$$y^q = \frac{z + \sqrt{z^2 - 4}}{2}, \quad y^{-q} = \frac{z - \sqrt{z^2 - 4}}{2},$$

or

$$u = y + y^{-1} = \frac{z^{\frac{1}{q}}}{2^{\frac{1}{q}}} \left[\left(1 + \sqrt{1 - \frac{4}{z^2}}\right)^{\frac{1}{q}} \right.$$

$$\left. + \left(1 - \sqrt{1 - \frac{4}{z^2}}\right)^{\frac{1}{q}} \right];$$

or developing

$$u = z^{\frac{1}{q}}\{S + T z^{-2} + U z^{-4} \ldots \&c.\}.$$

Hence the development of $x^{\frac{p}{q}} + x^{-\frac{p}{q}}$ in powers of $(x + x^{-1})$ or z, will necessarily contain negative powers of z, which has been just shown to be impossible.

To determine the coefficients A, B, C, &c. in the development (VI.).

As $z = x + x^{-1}$, we find by the binomial theorem,

$$\begin{aligned}
z^n &= x^n + n\,x^{n-2} + \frac{n\,.\,n-1}{1\ 2}x^{n-4} + \frac{n\,.\,n-1\,.\,n-2}{1\ 2\ 3}x^{n-6}, \ \&c.\\
z^{n-2} &= x^{n-2} + (n-2)\,x^{n-4} + \frac{(n-2)\ (n-3)}{1\ 2}x^{n-6}, \ \&c.\\
z^{n-4} &= x^{n-4} + (n-4)\,x^{n-6}, \ \&c.\\
z^{n-6} &= x^{n-6}, \ \&c.
\end{aligned}$$

Substituting for the powers of z in (VI.) their developments here given, and bringing over the terms on the left-hand side of the equation, we get

$$\begin{aligned}
0 = A&\left\{x^n + n\,x^{n-2} + \frac{n\,.\,n-1}{1\ 2}x^{n-4} + \frac{n.n-1\,.\,n-2}{1\ 2\ 3}x^{n-6}\ldots\right\}\\
-\,x^n + B&\left\{x^{n-2} + \frac{n-2}{1}x^{n-4} + \frac{n-2\ n-3}{1\ 2}x^{n-6}\ldots\right\}\\
&+ C\left\{x^{n-4} + \frac{n-4}{1}x^{n-6}\ldots\right\}\\
&+ \ \ldots \ D\left\{x^{n-6}\ldots\right\}.
\end{aligned}$$

Now the coefficients of the powers of x in this development are separately equal to cypher; and it is easy to show that the coefficient of x^{n-2t} or of the general term, in this development, is

$$\frac{A\,.\,n\,.\,n-1\,.\,n-2\ldots(n-t+1)}{1\ 2\ 3\ldots t} + \frac{B\,.\,n-2\,.\,n-3\ldots(n-t)}{1\ 2\ 3\ldots(t-1)}$$

$$+\frac{C\,.\,n-4\,.\,n-5\ldots(n-t-1)}{1\ 2\ 3\ldots(t-2)} + \frac{D.n-6\ldots(n-t-2)}{1\ 2\ldots(t-3)}\ \&c.$$

But this coefficient of x^{n-2t} may be thus written,

$$\left.\begin{array}{l} 0=\dfrac{n}{1\,.\,2\ldots t}\Big\{\mathrm{A}\,.\,(n-1)\,(n-2)\,.\,.\,(n-t+1) \\ \quad +\dfrac{\mathrm{B}\,t}{n}.(n-2)(n-3)\ldots(n-t)+\dfrac{\mathrm{C}\,t\,(t-1)}{n\,.\,n-3}(n-3) \\ \quad (n-4)\ldots(n-t-1)+\dfrac{\mathrm{D}\,t\,(t-1)\,(t-2)}{n\,.\,n-4\,.\,n-5}(n-4)\ldots \\ \quad (n-t-2)\ \&c.\ \&c.\Big\}\ .\ .\ .\ .\ .\ .\ .\ .\ . \end{array}\right\}\ \text{(VII.)}$$

Now the part of this series within the brackets is manifestly the same as (V.); comparing then the two series, term by term, we find

$$\mathrm{A}=1,\ \ \mathrm{B}=-n,\qquad \mathrm{C}=\frac{n\,.\,n-3}{1\,2},$$

$$\mathrm{D}=-\frac{n\,.\,n-4\,.\,n-5}{1\,2\,3},\ \ \mathrm{E}=\frac{n\,.\,n-5\,.\,n-6\,.\,n-7}{1\,2\,3\,4},\ \ \&c.\ \&c.$$

and the coefficient of z^{n-2t} is

$$\pm\frac{n\,(n-t-1)\,(n-t-2)\ldots(n-2\,t+1)}{1\,2\,3\ldots\ldots t}\ .\ \ .\ \text{(VIII.)}$$

When $2\,t=n$, the coefficient of z^0 becomes $\pm\,2$, as was shown above.

When n is odd, the coefficient of z is found by putting $2\,t=n-1$ in the general formula (VIII.), when it becomes $\pm\,n$.

We have now shown that

$$x^n+x^{-n}=z^n-n\,z^{n-2}+\frac{n\,.\,n-3}{1\,.\,2}z^{n-4}\ldots\&c.\ \ .\ \ \text{(IX.)}$$

or, as it may be more compendiously written,

$$x^n+x^{-n}=z^n\left[1\pm\sum\nolimits_1^{\frac{n}{2}}\left\{\frac{n.(n-t-1)\,(n-t-2)\ldots(n-2\,t+1)}{1\ 2\ 3\ldots t\ z^{2t}}\right\}\right]$$

Attributing to t all integer values from unity up to $\frac{n}{2}$ inclusive.

To develope the cosine of $n\,\theta$ in descending powers of $\cos\theta$.

Assume $2\cos\theta=x+x^{-1}$, then it may be easily shown that $2\cos n\,\theta=x^n+x^{-n}$. Substituting in (IX.), $2\cos n\,\theta$ for x^n+x^{-n}, $2\cos\theta$ for z, and dividing the whole equation by 2^n, we find

$$\left.\begin{aligned}\frac{\cos . n\theta}{2^{n-1}} &= \cos^n\theta - \frac{n\cos^{n-2}\theta}{1\ 2^2} + \frac{n . n-3}{1\ 2}, \frac{\cos^{n-4}\theta}{2^4} \\ &- \frac{n . n-4 . n-5}{1\ 2\ 3} . \frac{\cos^{n-6}\theta}{2^6} \ \ldots\ldots\end{aligned}\right\} . \text{(X.)}$$

The expansion of $\cos n\,\theta$, in powers of $\cos\theta$, fails when n is either negative or fractional, as $x^n + x^{-n}$ cannot be expanded in those cases in powers of z. It is also clear that the expansion can contain no negative powers of $\cos\theta$, as negative powers of z cannot appear in the development of $x^n + x^{-n}$.

To expand $\sin n\,\theta$ *in powers of* $\sin\theta$.

The investigation of this development must be divided into two cases, as n is odd or even. First let n be odd.

Put $\theta = \frac{\pi}{2} - \phi$, then $n\,\theta = \frac{n\,\pi}{2} - n\,\phi$, $\cos n\,\theta = \sin n\,\phi$, and $\cos\theta = \sin\phi$.

Making these substitutions in (X.), we find

$$\frac{\sin n\,\phi}{2^{n-1}} = \sin^n\phi - \frac{n}{1}\frac{\sin^{n-2}\phi}{2^2} + \frac{n . n-3}{1\ 2}\frac{\sin^{n-4}\phi}{2^4} \ldots \&c.\ \text{(XI.)}$$

Again, when n is even

Differentiating (X.), and dividing by $n\,d\,\theta$, we find

$$\left.\begin{aligned}\frac{\sin n\,\theta}{2^n} &= \sin\theta\left\{\frac{\cos^{n-1}\theta}{2^1} - \frac{n-2}{1}\frac{\cos^{n-3}\theta}{2^3}\right. \\ &\left. + \frac{n-3 . n-4}{1\ 2}\frac{\cos^{n-5}\theta}{2^5}.\right.\end{aligned}\right\} . \text{(XII.)}$$

Now let $\theta = \frac{\pi}{2} - \phi$, then $\sin n\,\theta = \mp \sin n\,\phi$, $\sin\theta = \cos\phi$, $\cos\theta = \sin\phi$. Making these substitutions in (XII.), there results

$$\begin{aligned}\mp\frac{\sin n\,\phi}{2^n} &= \cos\phi\left\{\frac{\sin^{n-1}\phi}{2^1} - \frac{n-2}{1}\frac{\sin^{n-3}\phi}{2^3}\right. \\ &\left. + \frac{n-3 . n-4}{1\ 2}\frac{\sin^{n-5}\phi}{2^5},\right.\end{aligned}$$

the upper sign being taken when n is divisible by 4, if otherwise, the lower.

XXIII. *On the Formation of the Cumulus Cloud. By* Thomas Hopkins, *Esq.**

IN the summer of last year, 1839, I went to the top of Snowdon, and found the whole of the mountain covered with a thick mist or cloud. While descending from the upper part of it, about two o'clock in the afternoon, I looked back and observed the summit quite free from cloud. In June last I again ascended the mountain, and again found it enveloped in the same kind of thick mist; but remembering that on my previous visit the mist had cleared away in the afternoon, I resolved to wait on the top. About two o'clock in the afternoon the mist began to break, driving along from the south-west and passing round the summit of the mountain. The wind was moderate, and the mist for some time concealed the country below to the north and the east; it, however, in a short time disappeared, and left the country open to view.

Towards the end of August I was at New Brighton and Waterloo near Liverpool, when the wind was also moderate and generally from the south, south-west, or west, and I made use of the opportunity which then presented itself, to notice the appearances of the clouds, and took notes at the time, of which the following are a sample:—

"*New Brighton, Aug.* 26, 1840.—At six in the morning a slight wind from the west; the sea covered with a darkish cloud, apparently resting on the water; the land also covered with low cloud, but it was less heavy than that over the sea. As the sun rose in the heavens the lower portion of the thick cloud disappeared from the part immediately over the sea, and cumulus clouds formed over the land, apparently near to the two chains of the Lancashire and Welsh mountains. These increased in size from about nine o'clock in the morning until one in the afternoon, and became fine massive clouds. There being a slight breeze from the west they slowly floated eastward, where they assumed a darker appearance. Other cumuli, which during the afternoon remained over the sea, rose higher in the atmosphere, and the tops gradually wasted away, the whole mass becoming elongated, and looking like large fish floating in the air. As the sun went down in the west these clouds became more like dark lines, and when the sun had set the sky soon became clear, and the stars shone out brightly."

For a fortnight afterwards the weather was similar in its general character, and it may be described as follows:—At sun-rise a thick mist rested on the Irish Sea, so as to conceal

* Communicated by the Author; having been read before the Literary and Philosophical Society of Manchester in 1840.

the greater part of it from view. Any ship that was on this part of the sea, and not too far off, seemed to be floating in the air. As the sun became more powerful, a transparent space appeared between the water and the mist above it, and the mist began to take more decidedly the appearance of dense cloud. This transparent space by degrees enlarged, and the cloud as it rose became nearly a straight line or stripe. Between eight and nine o'clock little irregular protuberances or cones began to appear on the upper portion of the stripe of cloud. These cones by degrees swelled and enlarged their sizes, both vertically and horizontally. By placing my eye against a fixed object and looking along a line, I could see the gradual swelling of the masses. They had rough irregular cauliflower-like tops, and the particular forms varied as they grew larger. The lower part of the cloud at the same time preserved its horizontal level, but became darker in appearance. This process continued until large cumuli were formed, extending over the sea apparently from the Welsh to the Westmoreland hills. While this was going on the whole mass rose, and by twelve o'clock was at a considerable elevation, the upper part of which looked like a number of heaps of fleecy cotton wool piled into irregular hills. Under the highest of these hills the cloud was the darkest, and the swelling movement in them resembled that seen in smoke issuing from a chimney, but was much slower. The light west or south-west wind, which generally prevailed, carried these masses of cloud eastward, where they seemed to accumulate and become darker. About noon, or shortly after, we commonly had two distinct ridges of fine bold cumuli, one extending across the sea, apparently from the great Orme's Head to the Westmoreland hills, and the other stretching over Wales and Cheshire to the Yorkshire hills. Recollecting the clouds which I had noticed forming about the sides and top of Snowdon, I was strongly impressed with the idea that these ridges came from Snowdon and the other mountains in its vicinity. About the same time of the day that I had formerly observed Snowdon to become clear from mist, namely, about two or three o'clock in the afternoon, these clouds generally ceased to come from that quarter, and those already formed rose higher, and seemed disposed to separate into detached masses. Later in the afternoon they became lighter, and generally dissolved gradually, the whole frequently disappearing a little after sunset. When the clouds were very heavy a different result was witnessed, as they joined together and gave out sprinklings of rain, and showers afterwards fell more or less frequently during the night. Over the lands of Lancashire and Cheshire

similar clouds formed, but they were more broken and irregular. At one time, when the morning mist had been raised, say apparently 100 or 150 yards above the surface, a most beautiful view was presented. An indefinite number of thin clouds seemed to be hung vertically in the air, of various degrees of transparency, with spaces intervening, so as to give a sufficiently distinct view of the lower part of each film of cloud. The whole looked like an immense hall with a mixture of glass and thin muslin drapery suspended in the upper part, the particular appearances changing every few minutes. Between these vertical films of cloud there appeared to be slight tremulous upward movements of the air, similar to those which are seen over heated sands or over the top of a hot stove, indicating that there were ascending currents of air in those places. Similar appearances were presented at other times, but they were less clear.

In watching such scenes, one of the most striking circumstances presented to the notice is the formation of the cumulus cloud. It takes so definite a form, has such an uniformity of character, appears so plainly to grow up before your eyes, and ascend from the surface of the land or water to a considerable elevation in the heavens, and is so grand in its appearance, that it becomes invested with a peculiar interest. The prevailing opinion which has existed for a long time past respecting the formation of clouds has, I believe, been, that they resulted from an intermixture of different currents of air, and that the small particles of water which constitute the cloud, notwithstanding appearances, do *not* in reality rise in opposition to the laws of gravity. But any one who will take the trouble to watch the formation of the cumuli over the Irish Sea, say from eight to twelve o'clock in the morning, towards the latter end of summer, will find it difficult to resist the conviction that they really *do* rise in the atmosphere. The same identical mass of cloud may often be distinctly traced from the surface of the sea until it passes over your head at a considerable elevation; and the swelling prominences of the upper part of the cloud when seen in profile are so like in shape and motion to those which may be seen in smoke issuing from a chimney, or in steam when escaping from a boiler, as to induce an impression that similar causes produce all the effects. Mr. Espy, in his lectures recently delivered in this town, professed to account for the formation of "the Cumulus;" but some gentlemen think he did so in an unsatisfactory manner, it therefore seems desirable that his theory should be subjected to the test of an examination. This gentleman (Mr. Espy) says, that the sun when it rises increases the temperature of that

part of the atmosphere which is near to the surface of the earth, and causes it to ascend to a greater elevation; this fact was previously known, and is, I presume, generally admitted. The part of the atmosphere near the earth which thus ascends has within it some certain quantity of steam or elastic aqueous vapour. When the air rises to an adequate height, and is consequently sufficiently cooled, a part of this steam is condensed. A new process now takes place. Upon the condensation of this portion of steam, latent caloric, or caloric of elasticity, is liberated, and this caloric attaches itself to surrounding bodies, and raises their temperature. In this way beds or strata of warm air are formed at different heights in the atmosphere.

But if liberated caloric raises the temperature of masses of air floating high up in the atmosphere, we know that those masses will expand through the increase of their elasticity. The additional elastic force thus acquired must be exerted in all directions, but as there will be the least resistance from above the force will be first developed principally in a vertical direction, and the whole column above will be raised, and caused to expand laterally in the higher regions of the atmosphere. On this second heating power, arising from condensation, being exerted, and a part of the atmosphere raised and thrown off laterally, the air below, relieved from a portion of the incumbent pressure, will spring upwards, and the surrounding air near the surface of the earth will flow in to restore the equilibrium of atmospheric pressure. Thus a local and temporary upward current will be created; and this upward current taking with it steam from near the surface of the earth, successive condensations take place—successive expansions follow; and the condensed steam, taking the shape of cloud, becomes a cumulus, and swells and grows in the way that has just been attempted to be described. The generally slow formation of the cloud, and the slight results which ordinarily follow, are, it is presumed, attributable to the limited portion of steam that is in the atmosphere. When steam is deficient in the raised air, no cloud is formed; when it is plentiful, clouds are freely formed; and when it is very abundant, the clouds are heavy and rain falls. The liberation of caloric by condensation it is evident may be effected in the smallest degree conceivable, and the process described may be slow, and the result only the elevation of the temperature of limited portions of the upper atmosphere, in the way in which the temperature of the tropical atmosphere is raised. Evaporation in the tropical regions furnishes a full supply of steam to the atmosphere, which steam being carried

upwards a slight condensation takes place, that increases the temperature and expansive force of the mass of air, and carries it into higher regions. This being done successively and constantly, the whole upper region is sufficiently warmed and expanded to carry the small condensed particles of water, the remaining steam, and the atmospheric gases to the proper height, where the whole flow over north and south towards the poles. The hazy atmosphere of the tropical seas is probably nothing more than an immense cumulus cloud, continually formed and carried away, north and south, the causes in operation being so regular as to prevent the cloud from being broken up or dispersed. At the outer edges of this tropical cloud disturbances take place similar to those which occur when cumuli are formed in the temperate regions. When the steam is very abundant and atmospheric disturbance great, the process which produces cumuli may go on and give rise to heavy rains, and possibly to storms; whether that process is always the sole or even principal cause in operation during storms is a subject worthy of investigation, but is one into which it is not proposed to enter at present.

It has been said by some persons who object to the theory here advanced, that when steam rises until it is cooled by expansion, sufficiently to produce condensation, upon the smallest particle of the latent heat of the steam being given out the cooling is stopped. They therefore contend that condensation *could not continue*, as it would be terminated at the very commencement of the process. And this would be true, if steam alone existed in the atmosphere. Were nothing present but steam it does not appear how there could be any considerable or continued rise of temperature from condensation, because any, the smallest, increase of the temperature must stop the condensation, and thus destroy the only source from whence is derived the increase of that temperature. But there are gases present with the steam and diffused through it, forming with it a minute mechanical intermixture. And when the condensation of steam liberates caloric, the liberated caloric attaches itself to the gases, as well as to the remaining portion of the steam, increases the expansive force of these gases, causes them to spring up, and to carry with them the steam that is diffused through and entangled among their particles. Steam, it will be remembered, forms but a small proportion of the whole atmosphere. When the temperature and dew-points are at 32° of Fahrenheit, steam constitutes but a $\frac{1}{240}$th part of the atmosphere; when they are at 52° it forms a $\frac{1}{120}$th part of it; when at 73° it is a $\frac{1}{60}$th part; and when at 80° it is only $\frac{1}{48}$th part. And as the dew-point in this part of the world is not

often above 52°, steam cannot be said to form at any time here much more than 1 per cent. of the atmosphere. The gases then being considered as 99, and steam as 1, the liberated caloric would attach itself to a much greater extent to the gases than to the remaining steam. The elasticity of the gases would be thereby increased; they would spring up, and carry the remainder of the steam with them to a sufficient height to cause fresh condensation to take place, and thus the process might be continued; not only is steam a very small portion of the whole atmosphere, but it cools according to a different law to that which regulates the cooling of the gases, and the difference in the laws by which the steam and the gases cool, causes the latter to cool the former. In ascending in the atmosphere, it is found that, when undisturbed by recent condensation, the temperature is reduced about 1° for every hundred yards of elevation. But the steam and the gases separately cool nearly according to the following table, taking whole numbers in a regular series as a sufficient approximation to the truth for our present purpose :—

At a height of about	1700 feet steam cools, say	1°,	while the gases cool	5°.
...	3400	...	2°	...	10°.
...	5000	...	3°	...	15°.
...	10,000	...	6°	...	30°.

Here we see that in an ascending column of steam and gases intermixed, while the former would by their ascension cool by expansion only one degree, the latter would cool five degrees; the gases, therefore, in their ascent would act the part of coolers to the steam. Being intimately intermixed the colder body would abstract heat from the warmer, and practically the gases when they rise must cool the steam that is intermixed with them. Then, as the steam is condensed more caloric is given out, and the gases again warmed and expanded; and thus these different elastic fluids, condensable and non-condensable, act and react upon each other, and give results different from those which would be found if they existed in the atmospheric space separately from each other. A portion of the atmosphere, nearly saturated with steam, being raised by a force acting from below to a height of 1700 feet, the steam within it would be cooled, according to its own law of cooling, 1°; but the gases, by their different law of cooling, would be cooled 5°; and as these fluids would be intimately intermixed, the lower temperature of the gases must, to a greater or less extent, be communicated to the steam, and a partial condensation of the latter must follow. This condensation we have seen would give out caloric, which, by raising the temperature of the remaining steam, would cer-

tainly have a tendency to prevent further condensation; but the temperature of the gases would also be raised, and their elastic force increased: they would therefore rise higher, and the remaining steam would be mechanically carried up with them sufficiently high to produce further condensation. That the liberated caloric would expand the gases enough to cause them to rise in the atmosphere is apparent, as it requires but little caloric to give considerable additional elasticity to the atmospheric gases. And on the condensation of steam much latent caloric is given out, there would therefore be an adequate supply of caloric to produce the effects named. It has been computed that the evolution of heat from the condensation of a pound of steam would be sufficient to raise the temperature of 3657 cubic feet of air 10°. Every pound of rain or cloud that is formed, will consequently raise the temperature of the atmosphere to that proportional extent.

This view, thowever, does not rest entirely on theoretical reasoning; that there are beds of warm air mixed with floating particles of water occasionally found in the atmosphere, is known from the experience of aëronauts, as well as from others who have ascended mountains. Mr. Green found such a bed at a height of nearly 10,000 feet from the earth, where the dew-point was at 64°, the same as at the surface of the earth at the time. At 11,060 feet, only 1·060 feet higher, the dew-point had fallen to 32°, only one half the temperature; because, there is no doubt, he had then got above the warming influence of condensation of steam. The effect of condensation in raising temperature, is seen also in the method practised of heating water by passing steam into it. It is found that one pound of steam will raise the temperature of 1000 pounds of water 1°. And Mr. Espy has shown, in the instrument he called "a Nephelescope," that rarefaction of the air, effected by removing pressure from it, cooled it after a certain rate or law; but this law of cooling was disturbed as soon as condensation took place. Condensation counteracted the cooling effect of expansion about one half, showing that if a column of air, in which steam had been recently condensed, were to be examined, it would be found that the temperature would be reduced, as we ascended in that column only one half of what it would be in a neighbouring column where no condensation had taken place. Suppose in the latter column the temperature to diminish 1° for every one hundred yards of elevation, as before shown, then, in the former column, where condensation had taken place, the temperature would be diminished only half a degree for every one hundred yards of height. If the foregoing facts and reasonings be admitted, it will follow that on con-

densation of steam taking place in the atmosphere there will be an upward atmospheric movement, which will cause fresh condensation and further upward movement, and this process may form the cumulus cloud, which may rise to a less or greater height in the atmosphere, and the cloud may be dissipated or may give out rain according to the quantity of steam contained in the atmosphere, and the condensing power of cold.

The atmospheric movements which have been here considered, are represented by Mr. Espy as originating with, and caused by, the direct heating power of the sun, but the action of the sun may be only one of the causes which produce the phænomena. Any cause which shall have the effect of elevating a column of the atmosphere sufficiently to cause condensation of steam to begin, may commence the process described, and cause cumuli to form. A current of air having nearly a maximum quantity of steam for the temperature, being forced by the inertia of the mass against a ridge of hills, the lower part must rise, and in rising must force up the whole of the column that rests upon it. This forcing up may be sufficient to raise the column high enough to condense some of the steam which it contains, when all the effects just described would take place in the same way as would have occurred, if the heating power of the sun, acting on the surface of the earth, had been the prime mover. In this way the ridges of mountains in Caernarvonshire and Merionethshire may force up the current of air flowing against them from the south-west, until the process of condensation commences and clouds are formed. And the long lines of cumuli flowing apparently from about Snowdon northward towards Westmoreland, or eastward towards Yorkshire, when they reach the English hills may get another lift, and the copious rains that fall in those parts may be thus accounted for. If mountains can force up atmospheric currents, it is evident that when different atmospheric currents move irregularly, one may force up another. On two currents meeting or crossing each other, one may force its way under the other, lift it up, and thus cause condensation to commence. In this way cumuli may be formed over land or sea, and rain may be discharged, and possibly even storms commenced, provided there be an adequate supply of steam for condensation; and all this may take place in the absence of the sun,—in the night as well as in the day. Should various currents moving in opposite directions penetrate each other at different elevations, it will follow, from what has been advanced, that clouds may be formed at the same time at different elevations, and each stratum of cloud may be affected

according to the quantity of steam contained in it, and the condensing power of the elevation to which it should be raised. In this part of the world cumuli generally come from the westward, because the winds from that quarter are more fully charged with steam than those from any other quarter. But they may be sometimes seen coming from the eastward over the Yorkshire hills, and these may have originated either in the elevating power of the sun, or of the hills. The passage of small cumuli across the heavens does not produce a sensible effect on the barometer. An alteration may be observed in the flatness of the top of the mercury when one of considerable size has been passing. Very large ones, dark at the under side, sometimes lower the mercury a little; but it seems to require one of great extent sufficient to cover the horizon, to cause a decided fall in the barometer.

XXIV. *Reply to* Prof. Challis, *on the Investigation of the Resistance of the Air to an Oscillating Sphere. By* G. B. Airy, *Esq., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I DID not intend to trouble you again on the subject of discussion between Professor Challis and myself. But the remarks of my esteemed friend, in his communication to your July Number, have tended so much to confuse the matter, and some of the doctrines which he has brought forward are (if I understand them rightly) so dangerous to the purity of mathematical reasoning, that I must crave your permission to place this series of remarks in your Journal. I promise that I will not occupy any more of your valuable space by continuing this discussion.

On the new equations discovered by Professor Challis as having claims for consideration, and marked by him (2.), (4.), (5.), page 65, I have only to remark, that I certainly supposed *every* person who knows anything of partial differentials (Professor Challis and myself included) to be fully aware that where there are n independent variables there are $\frac{n.\overline{n-1}}{2}$ such equations. But Professor Challis's language seems to imply that he is hardly aware that $\overline{n-1}$ of the equations are sufficient for nearly every purpose, and that they do contain absolutely and completely all the equations of the next order involving different independent variables in each differentia-

tion, and that (with due care in integration) they do therefore contain the remaining equations of the same order. Thus Professor Challis's equation (2.), or $\frac{d}{dz}\left(\frac{d\,\mathrm{P}}{dx}\right)=\frac{d}{dx}\left(\frac{d\,\mathrm{P}}{dz}\right)$ gives, on differentiation with respect to y, $\frac{d^2}{dy\,dz}\left(\frac{d\,\mathrm{P}}{dx}\right)$ $=\frac{d^2}{dx\,dy}\left(\frac{d\,\mathrm{P}}{dz}\right)$; but this same result may be obtained by differentiating my equations (1.) and (3.), or $\frac{d}{dy}\left(\frac{d\,\mathrm{P}}{dx}\right)$ $=\frac{d}{dx}\left(\frac{d\,\mathrm{P}}{dy}\right)$, and $\frac{d}{dz}\left(\frac{d\,\mathrm{P}}{dy}\right)=\frac{d}{dy}\left(\frac{d\,\mathrm{P}}{dz}\right)$. Professor Challis will find, on examination, that I have used all which were required.

But Professor Challis states that these six equations "are exactly verified by the kind of motion which Mr. Airy considers to be impossible" (page 64, line 29), and then proceeds to show (page 66) that Poisson's law of motion satisfies these equations. What particular obscurity in my expressions, or what omission of my explanatory words in Professor Challis's reading of them, can have led to such a misinterpretation of my meaning as this sentence implies, I cannot imagine. My words (page 322, line 15) were, "If, in order to support Professor Challis's expression for the movement of the particles to or from a centre, we suppose, &c. &c." I did not object to Poisson's expression, or to any other involving cos θ as a multiplier; but I remarked (page 328, line 17) that "the *onus* of proving that the three equations are consistent rests with the supporter, &c."; in other words, that the possibility is not to be assumed without proof, and that each case must rest on its own proof. That I did not intend to object to every expression involving cos θ as a multiplier in the formula for the resolved part of the velocity in the direction of radius, will be sufficiently evident from this circumstance; that the simplest of all kinds of motion, namely, that of a uniform current of air, in which all the particles move with the same velocity in parallel lines, is included in this case. My objection was specially to "*Professor Challis's* expression."

Professor Challis, in the proof that Poisson's assumed motion is possible, has preferred referring to the function ϕ, where $\frac{d\,\Phi}{dx}=u=$ velocity in the direction of x; $\frac{d\,\Phi}{dy}=v=$ velo-

city in the direction of y, $\frac{d\phi}{dz} = w =$ velocity in the direction of z. I have no objection to this (although it was inconvenient at first for the examination of Professor Challis's expression, because Professor Challis had not given the form of ϕ adopted by him), and, taking the general equations for ϕ as given by Professor Challis, I will show that Poisson's solution is tenable, and that Professor Challis's solution is untenable. The general equations are these:—

ϕ must be possible as a function of x, y, z, t,

$$\frac{d^2\phi}{dt^2} = a^2\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}\right).$$

Velocity in the direction of radius $= \frac{d\phi}{dr}$.

I proceed now with the substitution of the two expressions.

I. Poisson's Solution.

Here $$\phi = \left\{\frac{f(r-at)}{r^2} - \frac{f'(r-at)}{r}\right\}\cos\theta$$

$$= z\left\{\frac{f(r-at)}{r^3} - \frac{f'(r-at)}{r^2}\right\}.$$

Differentiating, and remarking that $\frac{dr}{dx} = \frac{x}{r}$, $\frac{dr}{dy} = \frac{dy}{dr}$, $\frac{dr}{dz} = \frac{z}{r}$,

$$\frac{d\phi}{dx} = zx\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$

$$\frac{d^2\phi}{dx^2} = z\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$
$$+zx^2\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-at)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-at)}{r^4}\right\}.$$

Similarly,

$$\frac{d^2\phi}{dy^2} = z\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$
$$+zy^2\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-at)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-at)}{r^4}\right\}.$$

Then

$$\frac{d\,\phi}{d\,z} = \left\{\frac{f(r-a\,t)}{r^3} - \frac{f'(r-a\,t)}{r^2}\right\} + z^2\left\{-\frac{3f(r-a\,t)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-a\,t)}{r^3}\right\}$$

$$\frac{d^2\,\phi}{d\,z^2} = 3\,z\left\{-\frac{3\,f(r-a\,t)}{r^5} + \frac{3\,f'(r-a\,t)}{r^4} - \frac{f''(r-a\,t)}{r^3}\right\} + z^3\left\{+\frac{15f(r-a\,t)}{r^7} - \frac{15f'(r-a\,t)}{r^6} + \frac{6f''(r-a\,t)}{r^5} - \frac{f'''(r-a\,t)}{r^4}\right\}.$$

The sum, or $\frac{d^2\,\phi}{d\,x^2} + \frac{d^2\,\phi}{d\,y^2} + \frac{d^2\,\phi}{d\,z^2}$ is

$$5\,z\left\{-\frac{3f(r-a\,t)}{r^5} + \frac{3\,f'(r-a\,t)}{r^4} - \frac{f''(r-a\,t)}{r^3}\right\} + z\,r^2\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-a\,t)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-a\,t)}{r^4}\right\}$$

$$= z\left\{\frac{f''(r-a\,t)}{r^3} - \frac{f'''(r-a\,t)}{r^2}\right\}.$$

But $\frac{d^2\,\phi}{d\,t^2} = a^2\,z\left\{\frac{f''(r-a\,t)}{r^3} - \frac{f'''(r-a\,t)}{r^2}\right\}.$

Therefore $\frac{d^2\phi}{d\,t^2} = a^2\left(\frac{d^2\,\phi}{d\,x^2} + \frac{d^2\phi}{d\,y^2} + \frac{d^2\phi}{d\,z^2}\right)$: and therefore Poisson's solution is possible.

II. Professor Challis's Solution.

Professor Challis has not, in his first papers, given a value for ϕ, but he has assumed (Phil. Mag. and Journal, December 1840), in page 463, line 25, "the velocity and density of the fluid which passes the area m^2 are v and ρ," and in page 465, line 7, &c. $v = \frac{f'(r-a\,t)}{r} - \frac{f(r-a\,t)}{r^2}$. From page 463, line 14, it is evident that this is the velocity in the direction of the radius: and from page 465, line 23, as also from page 131 (February 1841), line 6, that it is to have the factor $\cos\theta$, without which it cannot be adapted to the different points of the sphere's surface. Hence we have

$$\frac{d\phi}{dr} = \left\{\frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}\right\}\cos\theta$$

$$\phi = \frac{f(r-at)}{r}\cos\theta + \psi,$$

where ψ is a function of the angles which determine the position of r, and of t. If we make $\frac{x}{r} = l$, $\frac{y}{r} = m$, $\frac{z}{r} = n$, we may consider ψ as a function of l, m, n, t.

Using therefore $\phi = z\frac{f(r-at)}{r^2} + \psi\left(\text{since } \cos\theta = \frac{z}{r}\right)$,

$$\frac{d\phi}{dx} = zx\left\{-\frac{2f(r-at)}{r^4} + \frac{f'(r-at)}{r^3}\right\} + \frac{d\psi}{dx}$$

$$\frac{d^2\phi}{dx^2} = z\left\{-\frac{2f(r-at)}{r^4} + \frac{f'(r-at)}{r^3}\right\}$$

$$+zx^2\left\{+\frac{8f(r-at)}{r^6} - \frac{5f'(r-at)}{r^5} + \frac{f''(r-at)}{r^4}\right\}$$

$$+\frac{d^2\psi}{dx^2}.$$

Similarly,

$$\frac{d^2\phi}{dy} = z\left\{-\frac{2f(r-at)}{r^4} + \frac{f'(r-at)}{r^3}\right\}$$

$$+zy^2\left\{+\frac{8f(r-at)}{r^6} - \frac{5f'(r-at)}{r^5} + \frac{f''(r-at)}{r^4}\right\}$$

$$+\frac{d^2\psi}{dy^2}.$$

Then

$$\frac{d\phi}{dz} = \frac{f(r-at)}{r^2} + z^2\left\{-\frac{2f(r-at)}{r^4} + \frac{f'(r-at)}{r^3}\right\} + \frac{d\psi}{dz}$$

$$\frac{d^2\phi}{dz^2} = 3z\left\{-\frac{2f(r-at)}{r^4} + \frac{f'(r-at)}{r^3}\right\}$$

$$+z^3\left\{+\frac{8f(r-at)}{r^6} - \frac{5f'(r-at)}{r^5} + \frac{f''(r-at)}{r^4}\right\}$$

$$+\frac{d^2\psi}{dz^2}.$$

The sum, or $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}$ is

$$5z\left\{-\frac{2f(r-at)}{r^4} + \frac{f'(r-at)}{r^3}\right\}$$

$$+zr^2\left\{+\frac{8f(r-at)}{r^6}-\frac{5f'(r-at)}{r^5}+\frac{f''(r-at)}{r^4}\right\}$$

$$+\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}+\frac{d^2\psi}{dz^2}$$

$$=z\left\{-\frac{2f(r-at)}{r^4}+\frac{f''(r-at)}{r^2}\right\}+\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}+\frac{d^2\psi}{dz^2}.$$

But $\dfrac{d^2\phi}{dt^2}=a^2z\dfrac{f''(r-at)}{r^2}+\dfrac{d^2\psi}{dt^2}.$

And therefore $\dfrac{d^2\phi}{dt^2}-a^2\left\{\dfrac{d^2\phi}{dx^2}+\dfrac{d^2\phi}{dy^2}+\dfrac{d^2\phi}{dz^2}\right\}$

$$=2a^2z\frac{f(r-at)}{r^4}+\frac{d^2\psi}{dt^2}-a^2\left\{\frac{d^2\psi}{dx^2}+\frac{d^2\psi}{dy^2}+\frac{d^2\psi}{dz^2}\right\}.$$

It is necessary for the possibility of Professor Challis's solution, that the last expression be $=0$. The question therefore is, Can ψ be so determined as to make it $=0$? It clearly cannot. For, in order to make it $=0$, ψ must contain functions of $r-at$; but this is evidently inconsistent with its primary condition, that it should be a function of $\dfrac{x}{r}$, $\dfrac{y}{r}$, $\dfrac{z}{r}$, and t, and should contain r in no other way. Professor Challis's solution therefore is not possible.

In two or three passages (as page 64, line 21, and page 68, line 2), Professor Challis seems to intimate that there is a distinction to be made between the cases of motion in the direction of the radius from a fixed centre and motion in the direction of the radius from a moving centre. This distinction (as I understand it) is groundless; and for this reason, that the alteration which it makes in the place of a particle of air is of the *second* order with regard to the disturbance; and quantities of the second order are neglected through the whole investigation. Besides, it is evident that if the resolved part of the motion of the air, as measured from a fixed centre, is represented by $\cos\theta\times$ function of r, there will always be provided the proper vacuum of air to be occupied by the sphere (the oscillations being small). I trust, however, that I have misunderstood the meaning of the expressions.

Although I do not entirely agree with Professor Challis in attaching slight importance to this special problem, yet I consider the correctness or incorrectness of his result as almost insignificant when compared with the legitimacy or spurious-

ness of the process by which it has been obtained. With this estimation, I beg leave to call attention to the principle hinted at in page 67, line 22, &c. It will be remembered that ϕ is a function which is obtained by solving a partial differential equation, and that (as always happens) it contains in its expression another function f, whose form is arbitrary; *i. e.* any form may be used for f, provided that the quantity which is used as the subject of the function be that which is given by the solution, viz. $(r-at)$, and provided that the function chosen be placed exactly in the place of f in the solution, and be treated in no other way than that in which f is treated. Yet Professor Challis says, "The function ϕ may thus contain implicitly, *as a factor*, *another function* expressing the variation of velocity at a given instant in passing from one point to another in directions perpendicular to the motion, *but is not differentiated with respect to the variables of this factor.*" And a little further on, having found an expression

$\phi = \dfrac{f(r-at)}{r}$, he adds, "*the factor* $\cos\theta$, *depending on the mode of disturbance, being included in the arbitrary function.*" That is to say, having found that a certain set of differential equations is satisfied by the expression

$\phi = \dfrac{f(\sqrt{x^2+y^2+z^2}-at)}{\sqrt{x^2+y^2+z^2}}$, Professor Challis assumes that

we may use as a solution $\dfrac{z \cdot f(\sqrt{x^2+y^2+z^2}-at)}{x^2+y^2+z^2}$, and that

in substituting it in the differential equations we have no occasion to differentiate the factor $\dfrac{z}{\sqrt{(x^2+y^2+z^2)}}$. Against

this I must record my solemn protest. The function which is to be put in the place of the arbitrary function is simply to be put in its place, and to be modified in no other way whatever. If the solution in that form cannot be adapted to the mode of disturbance or to the initial motion, it is a sign that (among the infinity of solutions applying to fluids) one has been chosen which is inapplicable to the conditions of the problem, and another must therefore be tried. As to the omission of certain terms in the process of differentiation, I confess that I am surprised. Any person so well acquainted as Professor Challis is with the transformations of the equations for fluids, must be aware that the introduction ofthe peculiar function ϕ is a matter of convenience only; that it does not at all modify the suppositions on which the fundamental equa-

tions are obtained; that these fundamental equations are the four given by Professor Challis at the bottom of page 64; that they are established on consideration of the complete and absolute change in the state of pressure, density, and velocity, of the particles of the fluid, in proceeding from a point where the coordinates have one value to another point where they have a different value; and that it is absolutely impossible that there can be, in forming them, any limitation as to the functions whose change is or is not to be taken into account. I am quite certain that the new doctrine of omitting certain functions in the differentiation cannot stand a moment, when examined with reference to the original understanding on which the first equations have been obtained.

Perhaps the error of this principle may be made more obvious by considering a simpler case. Suppose we consider the motion of parallel plane waves through air. It is well known that the disturbance of a particle in the direction of x may be expressed by $X = \cos\alpha . \phi(vt - x\cos\alpha - y\cos\beta - z\cos\gamma)$. Suppose that there is a wall in the air, whose equation is $x = 0$. The value of X must therefore vanish when $x = 0$. But it does not vanish then. What must be done to make it vanish? A follower of Professor Challis would say, "Multiply it by a function which will vanish when $x = 0$, as for instance $a\,x$, and omit the variation of this factor in all differentiations." (This mode of escaping from the difficulty is not at all more forced than that adopted by Professor Challis in the passages to which I have adverted.) But I am sure that Professor Challis himself would say, "Not so; the process proposed is illegitimate; the failure of the expression (in its susceptibility of adaptation to the circumstances at the surface of the wall) is to be remedied, not by multiplying it by a function which vanishes there, but by altering the whole formula, so as to preserve the condition of satisfying the original differential equations, and also to satisfy the new condition relating to the wall." And thus he would obtain the formula

$$X = \cos\alpha . \phi(vt - x\cos\alpha - y\cos\beta - z\cos\gamma) \\ - \cos\alpha . \phi(vt + x\cos\alpha - y\cos\beta - z\cos\gamma).$$

I will only, in conclusion, express my regret at finding myself compelled to place myself so distinctly in opposition to my excellent friend Professor Challis. Had the writer been one of lower character or in a less influential position, or had the subject been one familiar to a greater number of mathematicians, I should have let it pass. But in observing the publication of principles the most dangerous to the *honesty* of mathematics (if I may use such an expression) that I have ever

seen, I could not avoid looking to the position of the writer, as well as to my own position, and forming my decision as to the course which was proper on my part. I have only to add, that nothing could be further from my intention than to give a personal character to this controversy, and that I trust no expression has escaped me which will bear such an interpretation.

I am, Gentlemen,
your obedient Servant,
G. B. Airy.

Royal Observatory, Greenwich,
July 10, 1841.

XXV. *On the Phænomena of Diffraction in the Centre of the Shadow of a Circular Disc, placed before a luminous Point, as exhibited by Experiment.* By Richard Potter, *Esq., M.A.**

THE results of the undulatory theory, to which I called attention in the Number of the Magazine for October last†, being opposed to the received opinions upon those points, it became desirable to undertake an experimental examination of the most decisive case; namely, the nature of the phænomena in the centre of the shadow of a circular disc when placed directly before a luminous point. According to that theory the point in the centre of the shadow ought to be as bright as if the disc were not interposed; and this being acknowledged on all sides, we have an admitted and clear test of the theory, as long as it is allowed to be a theory which may be tested by experiment.

The labour which the Astronomer Royal has devoted to deducing the numerical magnitudes, given in his paper in the January Number‡, has also rendered this experiment more interesting, and I may anticipate the result at which I have arrived, by stating that the undulatory theory in this, as in so many other cases, fails entirely.

Before relating the experiments, I must notice some of Mr. Airy's statements as to the visibility of luminous points, and the relative brightness which renders neighbouring spaces which are differently illuminated distinguishable from each other by the eye; because an incorrect assumption on these points would affect the whole discussion. To commence with the latter, he says, "Thus there will be at the centre a bright spot of double the general intensity, surrounded by rings brighter than the great expanse of light; but the excess of intensity in the rings, even the first, is so small, that it probably could not be seen." Now the intensity of the minima

* Communicated by the Author.
† Third Series, vol. xvii. p. 243. ‡ vol. xviii. p. 1.

being 1·, that of the maximum in the first bright ring is 1·16, in the second is 1·09, and in the third is 1·06.

To judge whether these or any of them could be seen, we may compare them with the expression for the relative intensities in Newton's rings seen by transmission, as given in Mr. Airy's tracts, page 305. This expression gives the ratio of the intensities in a dark and bright ring as 1· to 1·173, a ratio of the same order of magnitude as the above, which Mr. Airy assumes as representing relative intensities which probably could not be seen, although Newton's rings seen by transmitted light are a well-known conspicuous phænomenon. This also affects Mr. Airy's statement on a similar case in his paper "On the Theoretical Explanation of an apparent new Polarity in Light," in the Phil. Trans. for 1840. At p. 242, he says, "The destruction of bands is here as to sense complete," &c., where the ratio of the intensities in a minimum and a maximum were as 1· to 1·287 in one case, and as 1· to 1·098 in another, and yet in the former the difference of intensities is greater than in Newton's rings, which are admitted to be distinguishable. In Mr. Airy's opinions and experience, respecting the magnitudes which are sufficient to render luminous points visible, we have to notice the following. In his paper in the January Number of the Magazine, he says, "Substituting in the expression above, we find the diameter of the bright spot = 0·000308 inch, or less than $\frac{1}{3000}$ inch, regarding the visibility of which in common experience we need not to disquiet ourselves.

"If the diameter of the circular plate had been taken 1 inch, the distance of the screen remaining the same, the diameter of the spot would have been 0·0037 inch, a speck difficult even for a philosopher to discover under these circumstances. If the diameter of the plate were 0·1 inch, the diameter of the spot would be 0·037 inch, a very fit subject for experimental measure." In the fifth volume of the Cambridge Phil. Trans., p. 110, we find Mr. Airy saying, "I have used both parallelogrammic and circular holes of different sizes (the largest circular hole being $\frac{1}{4}$ inch in diameter), and have sometimes diminished the aperture to as little as $\frac{1}{1000}$ inch (by estimation.)" Thus we have Mr. Airy estimating the magnitude of *an aperture* of about $\frac{1}{1000}$ inch at one time, and at another time he would not have us disquiet ourselves as to the visibility of a *bright point surrounded by darkness*, when its diameter was as little as $\frac{1}{3000}$ inch. Yet we find the fixed stars are visible without having any sensible diameter; and the images of Jupiter's satellites formed in a telescope, with an object-glass of 30 inches focal length, have their diameters only about $\frac{1}{6000}$ inch, and

yet they are easily seen by such an eye-lens as in common experience we use in examining phænomena of diffraction: be it remembered these satellites are also bodies reflecting a very feeble light. Further, Mr. Airy says, a spot whose diameter is 0·0037 inch, or about $\frac{1}{300}$ inch, is "a speck difficult even for a philosopher to discover under these circumstances." Now $\frac{1}{300}$ inch is the breadth of an ordinary hair of the human head, and these are to be seen of a perceptible breadth, even as opake bodies *surrounded by opake bodies*: would not a philosopher both see, and expect to see, bright spots *when surrounded by darkness*, even if they were in diameter only a very small fraction of this magnitude?

To proceed to the immediate subject of the paper: I made preparation for trying the experiment several months ago, but when the apparatus was completed it was some time before a clear sunshine occurred to suit the aspect of the window of my darkened room, at a time when I could command leisure to pursue the experiments.

I prepared, by turning and polishing very carefully in a lathe, discs of brass of $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, $\frac{4}{10}$, and $\frac{7}{10}$ inch in diameter, and with watch-makers' implements one of $\frac{1}{20}$ inch diameter. The luminous point was the sun's image formed by a lens of $\frac{1}{6}$ inch focal length, and therefore it was of considerably less diameter than $\frac{1}{600}$ inch. The discs were attached by thin films of cement to a plate of glass with flat and parallel surfaces, which had never been before used, and they were so formed that the diffracting circular edge was raised from the glass; in some of them this edge was formed by the junction of the side and base of the frustum of a very obtuse-angled cone, and in others the edge was formed by the junction of two such frustums. The base by which they were attached to the glass was in all of much smaller diameter than the diffracting edge, so that the cement which oozed out round the edge of this base, was far out of the way of the light concerned in the phænomena of diffraction. The sun's light was reflected horizontally through a window-shutter of a darkened room, and the sun's image, formed by the lens of $\frac{1}{6}$ inch focal length, was at 60 inches distance from the disc, at the same time that the focus of the eye-lens, by which the phænomena of diffraction were examined, was at 60 inches distance on the other side. The plate of glass to which the discs were cemented, was rendered perpendicular to the incident light by adjusting until the reflected light passed directly back again. I generally used the apparatus so that the plate of glass was between the disc and luminous point, but I found no difference in the phænomena when it was re-

versed, or the disc between the plate and the luminous point. It was also found, on trial, that the appearances were not sensibly changed if the focus of the eye-lens were rather more or rather less than 60 inches from the disc.

When the whole was adjusted, on looking through an eye-lens of about 1 inch focal length, at the centre of the shadow cast by a disc, there was seen a bright central spot, of a white colour slightly tinged with brown, surrounded by a greater or less number of coloured rings, according to the size of the disc. The central bright spot in the shadow of the disc of $\frac{1}{20}$ inch diameter was large, and so bright, that at the first view it would have been taken to be equally bright with the light which had passed uninterruptedly, further from the edge of the disc than any interference was visible. But experience in photometrical experiments immediately pointed out to me the allowance to be made for the heightening effect of contrast, the bright spot being surrounded by a dark ring. It was evidently necessary to place the portions of light, which were to be compared, in the same circumstances. To effect this, I perforated a plate of thin sheet brass with the point of a needle, making a number of small circular holes of different sizes and at different distances, so that they could be taken in pairs of equal size. Now placing this brass plate in the focus of the eye-lens, where it was moveable and capable of adjustment, and causing one of these holes to transmit the *central part* of the central bright spot, and then comparing it with the light passing through an equal hole at a distance beyond any visible interference, it was seen at once that the brightness of the centre of the central bright spot in the shadow was very much less intense than that of the light passing through the other hole. In the shadow of the larger discs the inferiority of the brightness of the central spot was evident at once without any precautions, and the intensity appeared to diminish rapidly with the increase of diameter of the disc. The capability of the undulatory theory to explain the phænomena is thus completely set at rest; for, according to it, the brightness of the centre of the shadow should be equal to that of the uninterrupted light for all magnitudes of discs.

To obtain an estimate of the relative intensity of the light of the central spot for the disc of $\frac{1}{20}$ inch diameter, I adopted the following contrivance:—Having cut a number of small plates of mica out of the same sheet, I tried how many of these placed before the uninterrupted light reduced its intensity to an equality with that of the centre of the central spot, when the brass plate perforated with the small circular holes

was used. From the result of many trials I concluded that four plates produced the most accurate correspondence. To find how much light these mica plates transmitted, I looked through a rhomb of Iceland spar, at a strip of white paper so broad that the two images overlapped, and of course at such overlapping the intensity was the double of what it was on either side of it: a similar effect was produced by somewhere between two and three of the mica plates. I estimated that it was nearer two than three plates which were required; but to a scientific friend, with whom, on another occasion, I repeated it, it appeared to be nearer three than two that were requisite, so that I shall consider $2\frac{1}{2}$ of the mica plates as transmitting only one half of the light: and four such plates would therefore transmit one-third part very nearly. This shows how widely the fact was at variance with the result of the undulatory theory even with a disc of $\frac{1}{20}$ inch diameter.

To perform these experiments the eye-lens and brass plate in its focus were mounted on a stand so as to be raised or lowered at pleasure, and the mica plates, mounted in bundles of two, three and four, were attached to a rod of wood, so as to be interposed in the light in succession without inconvenience.

I have also the following memoranda: in the shadow of the $\frac{1}{20}$ inch disc, besides the central spot, two rings around it were visible; in that of the $\frac{1}{10}$ inch disc, four rings were visible; in that of the $\frac{2}{10}$ inch disc, five or six were visible; and in that of the $\frac{3}{10}$ inch disc, seven or eight were visible: and also as the diameter of the disc was increased, the intensity of the rings became more nearly equal to that of the central spot. The discs of $\frac{4}{10}$ and $\frac{7}{10}$ inch I did not use, as I must have provided larger plates of glass to attach them to, for the edge of the plates of glass to which the latter disc was attached caused interference sufficient to distort the rings when this plate was two inches square.

I must be allowed to state, that I consider the controversy, as to the undulatory theory being the physical theory of light, to be nearly terminated; and that the experiments necessary for completing the basis of a physical theory are those now most desirable to be undertaken.

Queen's College, June 9, 1841.

XXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xviii. p. 561.]

May 13, 1841. THE following papers were read, viz.—

1. "Meteorological Observations for August, September, and October, 1840, taken on board H.M.S. *Erebus* and *Terror*, by and under the direction of Capt. James Clark Ross, R.N., Commander of the Antarctic Expedition." Presented by the Lords Commissioners of the Admiralty, and communicated by the President of the Royal Society.

2. "Hourly Meteorological Observations made at Plymouth, in latitude 52° 36′ 12″, longitude in time 6^{m} 55^{s} east, on the 22nd of March, 1841." By Arthur Utting, Esq. Communicated by Capt. Edward Johnson, R.N., F.R.S.

3. "Barometrical Observations taken at Naples at 9 A.M. on each day during the months of January and February, 1841." By Sir Woodbine Parish, K.C.H., F.R.S. Presented by direction of the Council of the Royal Geographical Society, and communicated by S. H. Christie, Esq., Sec. R.S.

4. "Memoir of the case of a gentleman born blind, and successfully operated upon in the eighteenth year of his age; with Physiological Observations and Experiments." By J. C. August Franz, M.D., M.R.C.S. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

The young gentleman who is the subject of this memoir had been affected from birth with strabismus of both eyes; the right eye was amaurotic, and the left deprived of sight by the opacity both of the crystalline lens and of its capsule. At the age of seventeen, an operation for the removal of the cataract of the left eye was performed by the author with complete success. On opening the eye for the first time, on the third day after the operation, the patient described his visual perception as being that of an extensive field of light, in which everything appeared dull, confused, and in motion, and in which no object was distinguishable. On repeating the experiment two days afterwards, he described what he saw as a number of opake watery spheres, which moved with the movements of the eye, but when the eye was at rest remained stationary, and their margins partially covering one another. Two days after this the same phenomena were observed, but the spheres were less opake and somewhat transparent; their movements were more steady, and they appeared to cover each other more than before. He was now for the first time capable, as he said, of looking through these spheres, and of perceiving a difference, but merely a difference, in the surrounding objects. The appearance of spheres diminished daily; they became smaller, clearer, and more pellucid, allowed objects to be seen more distinctly, and disappeared entirely after two weeks. As soon as the sensibility of the retina had so far diminished as to allow the

patient to view objects deliberately without pain, ribands differently coloured were presented to his eye. These different colours he could recognize, with the exception of yellow and green, which he frequently confounded when apart, but could distinguish when both were before him at the same time. Of all colours, gray produced the most grateful sensation: red, orange and yellow, though they excited pain, were not in themselves disagreeable; while the effect of violet and of brown was exactly the reverse, being very disagreeable, though not painful. Brown he called an ugly colour: black produced subjective colours; and white gave rise to a profusion of *muscæ volitantes*. When geometrical figures of different kinds were offered to his view, he succeeded in pointing them out correctly, although he never moved his hand directly and decidedly, but always as if feeling with the greatest caution. When a cube and a sphere were presented to him, after examining these bodies with great attention, he said that he saw a quadrangular and a circular figure, and after further consideration described the one as being a square, and the other a disc, but confessed that he had not been able to form these ideas until he perceived a sensation of what he saw in the points of his fingers, as if he really touched the objects. Subsequent experiments showed that he could not discriminate a solid body from a plane surface of similar shape; thus a pyramid placed before him, with one of its sides towards his eye, appeared as a plane triangle.

Two months after the above-mentioned operation, another was performed on both eyes, for the cure of the congenital strabismus, by the division of the tendons of the recti interni muscles, which produced a very beneficial effect on the vision of the left eye; and even the right eye, which had been amaurotic, gained some power of perceiving light, and, from being atrophied, became more prominent. Still it was only by slow degrees that the power of recognizing the true forms, magnitudes, and situations of external objects was acquired. In course of time, the eye gained greater power of converging the rays of light, as was shown by the continually increasing capacity of distinct vision by the aid of spectacles of given powers*.

May 20.—The following papers were read, viz.—

1. "Catalogue of Geological Specimens procured from Kerguelen's Land during the months of May, June, and July, 1840."

2. "Catalogue of Birds collected on board Her Majesty's Ship *Terror*, between the Cape of Good Hope and Van Diemen's Land."

3. "Description of Plants from Kerguelen's Land, collected in May, June, and July, 1840."

The above papers are by John Robertson, Esq., Surgeon of Her Majesty's Ship *Terror*, and were presented to the Society by the Lords Commissioners of the Admiralty, and communicated by the President of the Royal Society.

4. "On the Fossil Remains of Turtles discovered in the Chalk

* In Phil. Mag., First Series, vol. xxviii. p. 203, will be found an account, by Sir E. Home, of two children born with cataracts, and the nature of their vision after the operations of depression and extraction.—Edit.

Formation of the South-East of England." By Gideon Algernon Mantell, Esq., LL.D., F.R.S.

In this paper the author gives a description, accompanied with drawings, of a remarkable fossil Turtle, referable to the genus *Emys*, and named from its discoverer, Mr. Bensted, the *Emys Benstedi*, which has been lately found in a quarry of the lower chalk of Kent, at Burham, which is situated near the banks of the Medway, between Chatham and Maidstone. The specimen discovered consists of the carapace or dorsal shell, six inches in length and nearly four inches in breadth, with some of the sternal plates, vertebræ, eight ribs on each side of the dorsal ridge, a border of marginal plates, and one of the coracoid bones. It is adherent to a block of chalk by the external surface of the sternal plates. The marginal plates are joined to each other by finely indented sutures, and bear the impress of the horny scales or tortoise-shell, with which they were originally covered. The expanded ribs are united together throughout the proximal half of their length, and gradually taper to their marginal extremities, which are protected by the plates of the osseous border. Mr. Bell considers the species to which it belonged as being closely allied in form to the common European Emys, and as possessing a truly fluviatile or lacustrine character. The plates of the plastron, however, as also the coracoid bone, resemble more the corresponding bones of marine than of freshwater turtles.

5. "Researches tending to prove the Non-vascularity of certain Animal Tissues, and to demonstrate the peculiar uniform mode of their Organization and Nutrition." By Joseph Toynbee, Esq. Communicated by Sir Benjamin Brodie, Bart., F.R.S., &c.

The above was only in part read.

May 27.—The following papers were read, viz.—

1. "On the Compensations of Polarized Light, with the description of a Polarimeter for Measuring Degrees of Polarization." By Sir David Brewster, K.H., D.C.L., F.R.S., and V.P.R.S. Ed.

In four papers published in the Philosophical Transactions for 1830, the author maintained, in opposition to the prevailing theory, that light, either reflected or refracted at angles different from that at which it is completely polarized, does not consist of two portions, one completely polarized, and the other completely unpolarized, but that every portion of it has the same physical property, having approximated in an equal degree to the state of complete polarization. This conclusion, which had been derived from reasoning on the hypothesis that a pencil of light, composed of two pencils polarized respectively at angles of + and − 45° with the plane of reflexion, was equivalent to a pencil of common light, is confirmed in this paper by experiment, made with common light itself, reflected from different parts of the atmosphere, and from which the uniaxal or biaxal systems of rings were obtained. On placing such a system between light partially polarized in an opposite plane, the author found that the rings disappeared, the direct system being seen on one side of the plane of disappearance, and the complementary system on the other side. In this experiment the polarization of the light

in one plane was compensated by the polarization of the same light in the opposite plane; and, consequently, both the pencils, which had undergone the two successive polarizing actions, had received the same degree of polarization in opposite planes; and in virtue of these two equal and opposite polarizations, the light at the point of compensation, where the system of rings disappeared, had been restored from partially polarized to common light; and the light on each side of this point of compensation was in opposite states of partial polarization.

In order to give a distinct view of the nature of this experiment, the author details the phenomena observed at particular angles of incidence on glass. From the results at an angle of incidence of 24°, the ray suffering one refraction at 80°, and a second reflexion at $83\frac{1}{2}$°, he concludes that the compensation which takes place is produced neither by an equality of oppositely polarized rays, nor by a proportional admixture of common light, but by equal and opposite physical states of the whole pencil, whether reflected or refracted.

The remarkable phenomena produced at an angle of incidence on glass of 82° 44′ (at which angle $\cos(i + i') = \cos^2(i + i')$), led the author to the construction of what he terms *the compensating rhomb*, consisting of a well-annealed rhomb of glass, or any other uncrystallized substance, having the angles of its base 130° 25′ and 46° 35′ respectively, when the index of refraction is 1·525. When a ray of light is incident upon the first surface at an angle of 82° 44′, exactly one-half of it is reflected; and the other half, after refraction, is reflected at the second surface, and emerges perpendicularly to the adjacent surface, without suffering refraction; each portion having, in the first instance, the same quantity of polarized light. The second portion is found, on examination, to be in the state of common light, although the ray at the second incidence consisted of more than one-half of polarized light. Hence if the pencil, previously to reflexion at the second surface, consist of 145 rays of polarized light, and 188 of common light, the effect of a single reflexion must be to depolarize polarized light, and to produce no change whatever upon common light; a property of a reflecting surface never yet recognized, and incompatible with all our present knowledge on the subject of the polarization of light.

The author then describes an instrument which he has invented for the purpose of accurately measuring the degrees of polarization, and which he therefore terms a *Polarimeter*. It consists of two parts; one of which is intended to produce a ray of compensation having a physical character susceptible of numerical expression, and the other to produce polarized bands, or rectilineal isochromatic lines, the extinction of which indicates that the compensation is effected. The construction and mode of operation of this instrument are, by the aid of figures, described and explained.

The following is the general law established by these researches; namely, that the compensations of polarized light are produced by equal and opposite rotations of the planes of polarization. Thus,

when a ray of common light is incident, at any angle, upon the polished surface of a transparent body, the whole of the reflected pencil suffers a physical change, bringing it more or less into a state of complete polarization, in virtue of which change its planes of polarization are more or less turned into the plane of reflexion; while the whole of the refracted pencil has suffered a similar, but opposite change, in virtue of which its planes of polarization are turned more or less into a plane perpendicular to the plane of reflexion.

The author then enters into a theoretical investigation of the subject, and concludes by pointing out a few of the numerous applications of his theory.

2. Continuation of the paper of which the reading commenced at the last Meeting, and entitled, "Researches tending to prove the Non-vascularity of certain Animal Tissues, and to demonstrate the peculiar uniform mode of their Organization and Nutrition." By Joseph Toynbee, Esq. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

In the introduction to this paper, the author first speaks of the process of nutrition in the animal tissues which are pervaded by ramifications of blood-vessels; pointing out the circumstance, that even in them there is a considerable extent of tissue which is nourished without being in contact with blood-vessels. The knowledge of this fact leads us to the study of the process of nutrition in the non-vascular tissues; which tissues he divides into the three following classes; namely, first, those comprehending articular cartilage, and the cartilage of the different classes of fibro-cartilage. Under the second head he comprises the cornea, the crystalline lens, and the vitreous humour; and, under the third, he arranges the epidermoid appendages; viz. the epithelium, the epidermis, nails and claws, hoofs, hair and bristles, feathers, horn and teeth.

The author then proceeds to show that the due action of the organs, into the composition of which these tissues enter, is incompatible with their vascularity. In proof of the non-existence of blood-vessels in these tissues, he states that he has demonstrated, by means of injections, that the arteries, which previous anatomists had supposed to penetrate into their substance, either as serous vessels, or as red-blood vessels too minute for injection, actually terminate in veins before reaching them; he also shows that around these non-vascular tissues there are numerous vascular convolutions, large dilatations and intricate plexuses of blood-vessels, the object of which he believes to be to arrest the progress of the blood, and to allow a large quantity of it to circulate slowly around these tissues, so that its nutrient liquor may penetrate into and be diffused through them. The author states that all the non-vascular tissues have an analogous structure, and that they are composed of corpuscles, to which he is induced to ascribe the performance of the very important functions in the process of their nutrition, of circulating throughout, and perhaps of changing the nature of the nutrient fluid which is brought by blood-vessels to their circumference. The author then brings forward facts in proof of the active and vital properties of these cor-

puscles, and concludes his Introduction by stating, that it appears to him, that the only difference in the mode of nutrition between the vascular and the non-vascular tissues is, that in the former, the fluid which nourishes them is derived from the blood that circulates throughout the capillaries contained in their substance; whilst, in the latter, the nutrient fluid exudes into them from the large and dilated vessels that are distributed around them: and that in both classes, the particles of which the tissues are composed derive from this fluid the elements which nourish them.

The author then enters on an examination of the structure and mode of nutrition of the several tissues of each of these three classes. In considering the first class, he commences with articular cartilage, which he describes at great length in the various stages of its development, and at the different periods of life. He gives in detail the account of numerous dissections of the ovum and fœtus illustrating the first stage, during which he shows that no blood-vessels enter into the substance of any of the textures composing a joint; but that the changes its component parts undergo, are effected by the nutrient fluid from the large blood-vessels, by which, at this stage, each articulation is surrounded. In the second stage of the development of articular cartilage, the author shows, by numerous dissections, the process by which the blood-vessels are extended into the substance of the epiphysal cartilage, and converge towards the attached surface of articular cartilage, and how, at the same time, blood-vessels are equally prolonged over a certain portion of its free surface. He shows that none of these blood-vessels enter the substance of the articular cartilage, and he points out that in them the arteries become continuous with the veins; first, by their terminating in a single vessel, from which the veins arise; secondly, by their forming large dilatations from which the veins originate; and, lastly, they become directly continuous with the veins in the formation of loops of various characters. In the third stage, that which is exhibited in adult life, the epiphysal cartilage is converted into osseous cancelli. These contain large blood-vessels, which are separated from the articular cartilage by a layer of bone composed of corpuscles, and the author believes that the principal source of nutrition to this tissue is the nutrient fluid which exudes into it from these vessels, by passing through the articular lamella just noticed. The free surface of adult articular cartilage is nourished by vessels which pass to a slight extent over it. The author points out the presence of fine tubes which pervade the attached portion of adult articular cartilage, to which he ascribes the function of transmitting through its substance the nutritive fluid derived from the vessels of the cancelli. He also advances the opinion that the articular cartilage becomes thinner during the whole of life, by being gradually converted into bone.

Fibro-cartilage constitutes the second tissue of the first class. The author first enters upon an examination of its structure; and in order to arrive at some definite conclusions on this subject, whereon anatomists of all ages have so much differed, he made numerous

dissections of fibro-cartilages in the different classes of animals at various periods of their development, the results of which he details. He arrives at the conclusion that this tissue is composed of cartilaginous corpuscles and of fibres; the latter preponderating in adult life, the former in infancy; and that during life the corpuscles are gradually converted into fibres. He enters at length into the question of the vascularity of these cartilages; and from a careful study of many injected specimens of man and animals at various periods of their development, the particular results of which he relates, he believes that blood-vessels are contained only in their fibrous portion, and have the function of nourishing that which is cartilaginous, and which, on account of its being subject to compression and concussion, does not contain any.

Among the second class of extra-vascular tissues, the cornea is first treated of; and its structure is described as being very lax, and as containing corpuscles only in a small quantity. The opinions in favour of its vascularity are combated; and it is shown that the blood-vessels which converge to its attached margin, and which are the principal source of the fluid that nourishes it, are large and numerous, and that at the circumference of this tissue the arteries, without any diminution of their calibre, return in their course, and become continuous with the veins. A second set of vessels, devoted to the nutrition of the cornea, is also described; they extend to a short distance over the surface of the tissue, but do not penetrate into its substance.

The crystalline lens is described as being composed of corpuscles, of which the radiating fibres are constituted. The arteria centralis retinæ is described as ramifying over the posterior surface of the capsule, where it forms large branches; these pass round the circumference of the lens, and reach its anterior surface, at the periphery of which they become straight: the arteries terminate in loops frequently dilated, and become continuous with the veins. With respect to the vascularity of the vitreous humour, the author states that although many anatomists have, in general terms, represented the arteria centralis retinæ as giving off, in its course through this organ, minute branches into its substance, still those who have paid especial attention to the subject, have not been able to find such vessels. He believes that the nutrition of this structure is accomplished by the fluid brought to its surface by the ciliary processes of the choroid, which fluid is diffused with facility through its entire substance by means of the corpuscles of which its membrane is composed, assisted by the semifluid character of the humour.

The third class of extra-vascular tissues comprehends the epidermoid appendages. The author describes them all as composed of corpuscles, which are round and soft where they are in contact with the vascular chorion, compressed and flattened where they are farther removed from it. He points out, in the substance of the hoof of the Horse, the existence of fine canals, which he supposes to conduct fluid through its mass; and he states that the perspiratory ducts of the human subject possess a structure analogous to the spiral vessels

of plants. The author describes each of the tissues of this class at length, and shows that the various modifications presented by the vascular system with which each is in contact, have the sole object of enabling a large quantity of blood to approach and circulate slowly around them. He also points out, in connexion with this subject, the remarkable vital properties which are possessed by these non-vascular tissues.

In concluding this paper, the author states that his object has been to establish as a law in animal physiology, that tissues are capable of being nourished, and of increasing in size, without the presence of blood-vessels within their substance. He shows the analogy which is presented between the extra-vascular animal and the extra-vascular vegetable tissues. He expresses a hope that the application to surgery of the above law, with reference to the prolongation of blood-vessels into the extra-vascular tissues during disease, and to pathology in the investigation of the nature of morbid structures, particularly of those classes which contain no blood-vessels, will be not devoid of interest, and will be productive of some advantage.

The Society then adjourned over the Whitsun recess, to meet again on the 10th of June.

June 10.—The following papers were read, viz.—

1. "Magnetic-term Observations made at Milan, on the 21st and 22nd of April, 1841." Communicated by Professor Carlini, For. Memb. R.S.

2. "Register of Tidal Observations made at Prince of Wales's Island, in July, August and September, 1840."

3. "Register of Tidal Observations made at Singapore in July, August and September, 1840."

These two papers were presented by the Directors of the East India Company, and communicated by P. M. Roget, M.D., Sec. R.S.

4. "On the Anatomy and Physiology of certain structures in the Orbit, not previously described." By J. M. Ferrall, Esq., M.R.I.A. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

The author describes a distinct fibrous tunic, which he terms the *tunica vaginalis oculi,* continuous with the tarsal cartilages and ligaments in front, and extending backwards to the bottom, or apex of the orbit; thus completely insulating the globe of the eye, and keeping it apart from the muscles which move it. The eye-ball is connected with this fibrous investment by a cellular tissue, so lax and delicate as to permit an easy and gliding motion between them. The use which the author assigns to this tunic is that of protecting the eye-ball from the pressure of its muscles while they are in action. This tunic is perforated at its circumference, and a few lines posterior to its anterior margin, by six openings, through which the tendons of the muscles emerge in passing to their insertions, and over which, as over pullies, they play in their course. A consequence of this structure is that the recti muscles become capable of giving rotatory motions to the eye without occasioning its retraction within the orbit, and without exerting injurious pressure on that organ. In those animals which are provided with a proper retractor muscle,

the recti muscles are, by means of this peculiar mechanism, enabled to act as antagonists to that muscle.

5. "An account of some recent improvements in Photography." By H. F. Talbot, Esq. F.R.S.

The author had originally intended, in giving an account of his recent experiments in photography, to have entered into numerous details with respect to the phænomena observed; but finding that to follow out this plan would occupy a considerable time, he has thought that it would be best to put the Society, in the first place, in possession of the principal facts, and by so doing perhaps invite new observers into the field during the present favourable season for making experiments. He has, therefore, confined himself at present to a description of the improved photographic method, to which he has given the name of *Calotype**, and reserves for another occasion all remarks on the theory of the process.

The following is the method of obtaining the Calotype pictures.

Preparation of the Paper.—Take a sheet of the best writing paper, having a smooth surface, and a close and even texture.

The water-mark, if any, should be cut off, lest it should injure the appearance of the picture. Dissolve 100 grains of crystallized nitrate of silver in six ounces of distilled water. Wash the paper with this solution, with a soft brush, on one side, and put a mark on that side whereby to know it again. Dry the paper cautiously at a distant fire, or else let it dry spontaneously in a dark room. When dry, or nearly so, dip it into a solution of iodide of potassium containing 500 grains of that salt dissolved in one pint of water, and let it stay two or three minutes in this solution. Then dip it into a vessel of water, dry it lightly with blotting-paper, and finish drying it at a fire, which will not injure it even if held pretty near: or else it may be left to dry spontaneously.

All this is best done in the evening by candlelight. The paper so far prepared the author calls *iodized paper*, because it has a uniform pale yellow coating of iodide of silver. It is scarcely sensitive to light, but, nevertheless, it ought to be kept in a portfolio or a drawer, until wanted for use. It may be kept for any length of time without spoiling or undergoing any change, if protected from the light. This is the first part of the preparation of Calotype paper, and may be performed at any time. The remaining part is best deferred until shortly before the paper is wanted for use. When that time is arrived, take a sheet of the *iodized paper* and wash it with a liquid prepared in the following manner:—

Dissolve 100 grains of crystallized nitrate of silver in two ounces of distilled water; add to this solution one-sixth of its volume of strong acetic acid. Let this mixture be called A.

Make a saturated solution of crystallized gallic acid in cold distilled water. The quantity dissolved is very small. Call this solution B.

When a sheet of paper is wanted for use, mix together the liquids A and B in equal volumes, but only mix a small quantity of them at

* See our last Number, p. 88.—Edit.

a time, because the mixture does not keep long without spoiling. I shall call this mixture the *gallo-nitrate of silver*.

Then take a sheet of *iodized paper* and wash it over with this *gallo-nitrate of silver*, with a soft brush, taking care to wash it on the side which has been previously marked. This operation should be performed by candlelight. Let the paper rest half a minute, and then dip it into water. Then dry it lightly with blotting-paper, and finally dry it cautiously at a fire, holding it at a considerable distance therefrom. When dry, the paper is fit for use. The author has named the paper thus prepared *Calotype paper*, on account of its great utility in obtaining the pictures of objects with the camera obscura. If this paper be kept in a press it will often retain its qualities in perfection for three months or more, being ready for use at any moment; but this is not uniformly the case, and the author therefore recommends that it should be used in a few hours after it has been prepared. If it is used immediately, the last drying may be dispensed with, and the paper may be used moist. Instead of employing a solution of crystallized gallic acid for the liquid B, the *tincture of galls* diluted with water may be used, but he does not think the results are altogether so satisfactory.

Use of the Paper.—The *Calotype paper* is sensitive to light in an extraordinary degree, which transcends a hundred times or more that of any kind of photographic paper hitherto described. This may be made manifest by the following experiment:—Take a piece of this paper, and having covered half of it, expose the other half to daylight for the space of *one second* in dark cloudy weather in winter. This brief moment suffices to produce a strong impression upon the paper. But the impression is latent and invisible, and its existence would not be suspected by any one who was not forewarned of it by previous experiments.

The method of causing the impression to become visible is extremely simple. It consists in washing the paper once more with the *gallo-nitrate of silver*, prepared in the way before described, and then warming it gently before the fire. In a few seconds the part of the paper upon which the light has acted begins to darken, and finally grows entirely black, while the other part of the paper retains its whiteness. Even a weaker impression than this may be *brought out* by repeating the wash of gallo-nitrate of silver, and again warming the paper. On the other hand, a stronger impression does not require the warming of the paper, for a wash of the gallo-nitrate suffices to make it visible, without heat, in the course of a minute or two.

A very remarkable proof of the sensitiveness of the Calotype paper is afforded by the fact stated by the author, that it will take an impression from simple moonlight, not concentrated by a lens. If a leaf is laid upon a sheet of the paper, an image of it may be obtained in this way in from a quarter to half an hour.

This paper being possessed of so high a degree of sensitiveness, is therefore well suited to receive images in the camera obscura. If the aperture of the object-lens is one inch, and the focal length

fifteen inches, the author finds that *one minute* is amply sufficient in summer to impress a strong image upon the paper of any building upon which the sun is shining. When the aperture amounts to one-third of the focal length, and the object is very white, as a plaster bust, &c., it appears to him that *one second* is sufficient to obtain a pretty good image of it.

The images thus received upon the Calotype paper are for the most part invisible impressions. They may be made visible by the process already related, namely, by washing them with the gallo-nitrate of silver, and then warming the paper. When the paper is quite blank, as is generally the case, it is a highly curious and beautiful phænomenon to see the spontaneous commencement of the picture, first tracing out the stronger outlines, and then gradually filling up all the numerous and complicated details. The artist should watch the picture as it developes itself, and when in his judgment it has attained the greatest degree of strength and clearness, he should stop further progress by washing it with the fixing liquid.

The fixing process.—To fix the picture, it should be first washed with water, then lightly dried with blotting paper, and then washed with a solution of *bromide of potassium*, containing 100 grains of that salt dissolved in eight or ten ounces of water. After a minute or two it should be again dipped in water and then finally dried. The picture is in this manner very strongly fixed, and with this great advantage, that it remains transparent, and that, therefore, there is no difficulty in obtaining a copy from it. The Calotype picture is a *negative* one, in which the lights of nature are represented by shades; but the copies are *positive*, having the lights conformable to nature. They also represent the objects in their natural position with respect to right and left. The copies may be made upon Calotype paper in a very short time, the invisible impressions being *brought out* in the way already described. But the author prefers to make the copies upon photographic paper prepared in the way which he originally described in a memoir read to the Royal Society in February 1839*, and which is made by washing the best writing paper, *first* with a weak solution of common salt, and *next* with a solution of nitrate of silver. Although it takes a much longer time to obtain a copy upon this paper, yet when obtained, the tints appear more harmonious and pleasing to the eye; it requires in general from 3 minutes to 30 minutes of sunshine, according to circumstances, to obtain a good copy on this sort of photographic paper. The copy should be washed and dried, and the fixing process (which may be deferred to a subsequent day) is the same as that already mentioned. The copies are made by placing the picture upon the photographic paper, with a board below and a sheet of glass above, and pressing the papers into close contact by means of screws or otherwise.

After a Calotype picture has furnished several copies, it sometimes grows faint, and no more good copies can then be made from

* Printed in Phil. Mag., Third Series, vol. xiv. p. 209.—Edit.

it. But these pictures possess the beautiful and extraordinary property of being susceptible of revival. In order to revive them and restore their original appearance, it is only necessary to wash them again by candlelight with gallo-nitrate of silver, and warm them: this causes all the shades of the picture to darken greatly, while the white parts remain unaffected. The shaded parts of the paper thus acquire an opacity which gives a renewed spirit and life to the copies, of which a second series may now be taken, extending often to a very considerable number. In reviving the picture it sometimes happens that various details make their appearance which had not before been seen, having been latent all the time, yet nevertheless not destroyed by their long exposure to sunshine.

The author terminates these observations by stating a few experiments calculated to render the mode of action of the sensitive paper more familiar.

1. Wash a piece of the *iodized paper* with the gallo-nitrate; expose it to daylight for a second or two, and then withdraw it. The paper will soon begin to darken spontaneously, and will grow quite black.

2. The same as before, but let the paper be warmed. The blackening will be more rapid in consequence of the warmth.

3. Put a large drop of the gallo-nitrate on one part of the paper and moisten another part of it more sparingly, then leave it exposed to a very faint daylight; it will be found that the lesser quantity produces the greater effect in darkening the paper; and in general, it will be seen that the most rapid darkening takes place at the moment when the paper becomes nearly dry; also, if only a portion of the paper is moistened, it will be observed that the edges or boundaries of the moistened part are more acted on by light than any other part of the surface.

4. If the paper, after being moistened with the gallo-nitrate, is washed with water and dried, a slight exposure to daylight no longer suffices to produce so much discoloration; indeed it often produces none at all. But by subsequently washing it again with the gallo-nitrate and warming it, the same degree of discoloration is developed as in the other case (experiments 1 and 2). The dry paper appears, therefore, to be equal, or superior in sensitiveness to the moist; only with this difference, that it receives a *virtual* instead of an *actual* impression from the light, which it requires a subsequent process to develope.

5. " New mode of preparation of the Daguerréotype plates, by which portraits can be taken in the short space of time of from five to fifteen seconds, according to the power of light, discovered by A. Claudet in the beginning of May 1841." Communicated by the Marquis of Northampton, Pres. R.S.

" My improvement," says the author, " consists in using for the preparation of the plates, a combination of chlorine with iodine, in the state of chloride of iodine. I follow the preparation recommended by Daguerre. After having put the plate in the iodine box for a short time, and before it has acquired any appearance of yellow co-

lour, I take it out, and pass it for about two seconds over the opening of a bottle containing chloride of iodine; and immediately I put it again in the iodine box, where it acquires very soon the yellow colour, which shows that the plate is ready to be placed into the camera obscura. I have substituted to the chloride of iodine, chloride of bromine, and have found nearly the same result; but I prefer chloride of iodine as producing a better effect; and besides, on account of the noxious smell of bromine.

"The result of my preparation is such, that I have operated in ten seconds with the same apparatus, which, without any chlorine, required four or five minutes; when using only the original preparation of Daguerre, I have obtained an image of clouds in *four seconds.*"

GEOLOGICAL SOCIETY.

[Continued from vol. xviii. p. 590.]

Dec. 16, 1840.—A paper "On the Relative Connection of the Eastern and Western Chalk Denudations," by P. J. Martin, Esq., F.G.S., was read.

The author advances this as the first of a series of papers on the construction of that part of the country usually considered as appertaining to the great chalk denudation of the Weald, or more properly, the upburst of the secondary formations between the tertiary of the respective basins of London and Hampshire.

In venturing on this field of inquiry, he professes also to take up the subject where it was left by him in two former memoirs, one published in 1828 under the title of a 'Geological Memoir of Western Sussex, with some Observations on Chalk Basins and the Weald Denudation*,' the other in the 'Philosophical Magazine' for February 1829†; and to extend the number of demonstrative facts that bear upon the theory of denudation by disruptive violence and contemporaneous aqueous abrasion, there brought forward as a corollary to Dr. Buckland's theory of 'Valleys of Elevation.'

In pursuance of this object, he begins by an examination into the arrangement of the great chalk dome of Hampshire and Wiltshire,—the *Patria* of the chalk of Pennant and Conybeare; its anticlinal lines of disturbance or upheaval, and their connections with those of the Weald and the smaller western denudations of Pewsey, Wardour and Warminster.

He finds that six great anticlinal lines are the main instruments of the upbearing of this abraded chalk; that the three which characterize the smaller anticlinal western valleys are projected onward, and in a manner decussate three others which emanate from the western extremity of the greater valley of the Weald, the vale of Wolmar Forest, from whence he starts his inquiry; and that these lines do not inosculate or enter into each other; approxima-

* An analysis of this memoir appeared in Phil. Mag., Second Series, vol. iv. p. 38.—Edit.

† Second Series, vol. v. p. 111.

ting, indeed, but little in any part of their course; severally dying out, and their respective synclinal lines playing off into each other. Their course is rather irregular, and their force exceedingly variable; but their general parallelism is maintained throughout, their progress being E. and W., with a point to the N.

The Pewsey line, after passing through the valleys of Ham and Kingsclere, is traceable between Woolverton and Hannington, on towards Monks Sherborne, and fades away at Old Basing, apparently without entering the tertiary beds of the London basin *. This meets in synclinal relation with a line projected from the north-west corner of the Wolmar valley from Pease Marsh, near Guildford, through Farnham and the high chalk range of Froyle, Shaldon, Dummer and Popham, and appears to fade away in the country west of Andover, where it is lost in the greater swell of the Burghclere Hills, and the more dominant power of the Pewsey upheaval.

The anticlinal line of Wardour, left by Dr. Fitton (in his 'History of the Beds below the Chalk' †) at Harnham Hill, S. of Salisbury, Mr. Martin finds traceable eastward, north of Dean Hill, and east of the Avon, to the banks of the Test, where it dips under the tertiary beds between Michaelmarsh and Romsey, and appears to fade away between the above-mentioned river and the Itching. In synclinal relation this line is also met and passed by a very remarkable anticlinal, traceable in strict approximation with, and by-and-by to be proved to be the *proximate cause* of, the whole line of the South Down escarpment (with a small exception between Lewes and Poynings) from Beachy Head to East Meon. In the vicinity of this place, at Langrish, it enters the chalk, passes through the anticlinal valley of Chilcomb near Winchester and that city, and is lost in the Bossington Hills, pointing towards, but not satisfactorily traced into, the Warminster line.

The details of all three lines of elevation are made out in the Ordnance Map, and sections given of the most illustrative points: and Mr. Martin adds some observations respecting the entrance of the great central line of elevation of the Weald into the chalk at Selborne, and its progress westward between the lines of Peasemarsh on the north, and of Greenhurst or the South Down on the south, till it fades away in the great plateau of Salisbury Plain.

The author concludes this paper with some reference to the subject of transverse fractures in these several longitudinal fissures, and the cross drainage, to which, like that of the Weald, he proposes to return, in extension and emendation of the disquisitions formerly published by him, as above alluded to, and which will be adduced as illustrative of the strong probability, if they do not amount (in

* The author thinks, that although this line fades away as it enters the tertiary beds at Old Basing, it is probable that, after passing silently along the London basin, it is revived again in the Isle of Thanet, which is a chalk outlier, by protrusion; in the same way that the parallel line of Portsdown Hill, High-down, near Worthing, and the Seaford Cliff (figured by Dr. Mantell) does on the southern coast.

† Geol. Trans., Second Series, vol. iv. p. 244. *et seq.*

connexion with the phænomena of drift) to absolute proof, of the close relation of the acts of upheaval and violent aqueous abrasion. This necessarily implies the belief that the date of these lines of disturbance is posterior to that of all the stratified beds of the south-east part of England, as maintained in the author's former essays, but into the full discussion of which he declines to enter till the whole subject is before the Society.

Jan. 6, 1841.—A paper was first read "On the Illustration of Geological Phænomena by means of Models," by Thomas Sopwith, Esq., F.G.S.

Mr. Sopwith commences by stating, that drawings cannot convey to the mind a correct notion of geological phænomena where more planes than one are required; and that few persons are aware of the extraordinary changes which are produced in the combinations of strata by viewing them on different planes, especially if the strata are dislocated; or in undisturbed and parallel strata, of the totally different apparent forms which are exhibited on an undulating surface and in any plane section of the interior. This difference is of great importance in mining plans, where the surface only in the first instance is accessible to examination, and the observer is too apt to infer from it, the subterranean relations of the strata. To convey clear notions of these differences Mr. Sopwith has prepared a series of hand models, about two inches square, formed of layers of differently coloured woods, and capable of being dissected, to a certain extent, vertically, obliquely or horizontally. They are also so shaped on the upper surface as to exhibit the undulations of the ground. It is not possible to convey verbally, a clear notion of the many curious combinations exhibited by these models.

A paper was next read, "On the Geology of the island of Madeira," by James Smith, Esq., of Jordan Hill, F.G.S.

The crust of that island, Mr. Smith states, is composed to the depth of several thousand feet of subaërial volcanic matter, erupted during the tertiary period; and he adds, perhaps no other volcanic region offers more favourable opportunities for investigation. To account for its rugged and fragmentary character, it is not necessary, he says, to have recourse to the supposition that Madeira is one of the remains of a vast continent, as there are in the volcanic constitution of the island, and in the action of the mountain torrents, sufficient elements to afford data for explaining every physical phænomenon.

The igneous rocks composing the greater part of the island, are lavas, sand, and ashes, with bombs, lapilli, pumice, volcanic scoriæ or cinders, tufas and conglomerates; and the non-igneous, the limestone of San Vincente, the coal or lignite of San George, and the sands of Caniçal.

Volcanic Rocks.

The lavas are wholly basaltic, containing numerous crystals of olivine; and they are compact, scoriaceous and vesicular.

The compact variety occurs in beds or *coulées* interstratified with

the other volcanic products, and in dykes which intersect all the igneous rocks. It is occasionally amorphous, but more often rudely columnar; it also, though rarely, presents regular columns; and it is sometimes schistose, possessing planes of cleavage as well as of regular stratification.

The scoriaceous basalt is rough and porous, resembling the slag of a foundry. Where the bed is thin it is scoriaceous throughout, but where it is of a certain thickness only the upper and lower surfaces exhibit this character. Two caverns of considerable magnitude occur immediately to the west of the principal landing-place at Funchal, and there are others in the island.

The vesicular lava or basalt presents through its whole mass a porous texture. The large vesicles have been flattened by the gravity of the lava, and elongated in the direction in which the *coulée* flowed. Where they are numerous and minute, they permit the rock to be easily hewn; and this variety is called *cantaria rija*, or the hard building stone.

The lapilli, sand, ashes and volcanic bombs, appear to have been projected simultaneously, as the bombs were evidently half imbedded in the finer materials by the force of their fall, the laminæ beneath them being bent upwards, and in some instances to a greater height on one side than the other, indicating the direction in which the bombs fell.

The pumiceous lapilli are white or light yellow, and rarely exceed in size a pigeon's egg. Beds of pumice, varying in thickness from a few inches to several feet, occur either on the surface or interstratified with the basalt and tufa; and they often contain portions of heavier volcanic products, as cinders or scoriæ, dispersed without regard to gravitation, proving, Mr. Smith says, that these various materials could not have been deposited under the sea, because in water they would instantly have separated according to their respective weights. The scoriæ or cinders also form extensive beds. They are generally reddish, and vary in size according to the distance from the orifice of eruption.

The ashes, both dark and light-coloured, are incoherent, except where they are mixed with earthy matter, or apparently fell on a heated cone of eruption; and in these cases they form a scoriaceous mass. Tufas and conglomerates compose a large proportion of the volcanic rocks of Madeira, and are considered to owe their consistency to water. Fragments of vegetables are not uncommon in them, but Mr. Smith is not aware that they contain any other organic remains. Many of these beds have been converted into vegetable soils, and it is interesting, the author says, to observe the roots of plants still in the attitude in which they grew; and to witness traces of the very same phænomena which are now taking place at the surface, in strata which have been buried for so many ages under solid rocks. The remains of plants are chiefly found in the vegetable soils, but their roots occasionally occur in the hard rock, and the cracks or fissures are in many cases filled with closely-matted masses of what was once roots and fibres, but now consist of carbonate of

lime. Where the soils have been overflowed with lava, the vegetable remains are charred, and the soils have been burnt to the colour and hardness of brick; and if the overlying lava is of great thickness, a columnar structure appears. This conversion of soil into brick has been observed in the Azores and elsewhere.

The principal chain of mountains must at one time, Mr. Smith states, have been much higher, because their very summits consist of beds which are met with only at the base of active volcanic cones. There is consequently no great crater in the island, but there are the ruins of several truncated craters, and many small lateral cones. The most extensive of the former is the Curral dos Freiras, an immense ravine about three miles in length, one in breadth, and 2000 feet in depth, and open on its southern side. The beds of basalt, tufa and ashes of which it is composed, dip outwards to the base of the mountain, and parallel to its surface. Mr. Smith is convinced, the volcanic products of the island being subaërial, that this is not a crater of elevation, though it agrees with the characters which have been assigned to such craters; and he is further induced to infer, from the resemblance of the Curral dos Freiras to the more ancient portions of Teneriffe and the other Canary Islands, said to be craters of elevation raised from beneath the level of the sea, that a wrong conclusion has been drawn respecting them. He does not object to there being elsewhere true craters of elevation.

The principal lateral cones are to the west of Funchal, but they are in general so completely blotted with vegetation that their structure is concealed. In the ditch of a fort constructed on one of them, called Pico de St. João, a scoriaceous conglomerate intersected by minute basaltic veins is exposed; and a similar conglomerate occurs in the fortified island in Funchal Bay, also at the eminence at the landing-place. Some of these cones are covered by beds of lava and tufa erupted from craters at Cape Giram, in one instance to the thickness of 1400 feet. The beauty and regularity, within limited distances, of these volcanic strata, and the richness and variety of their colouring, are exceedingly striking. The most remarkable volcanic series, amounting to many hundred beds, is at Cape Giram, the cliff, 1600 feet in height, being stratified from the base to the summit. It has been rent in many places, and the fissures which terminate upwards in acute angles, have been filled with lava ejected from below.

Non-volcanic Rocks.

The limestone of San Vincente, Bowditch considered to be transition, but Mr. Smith shows that it belongs to the tertiary epoch, yet he believes it to be the fundamental rock of the island. It crosses a mountain stream between 2000 and 3000 feet above the level of the sea, and abounds in zoophytes and marine testacea belonging to the genera *Cardium, Pecten, Pectunculus, Spondylus, Cypræa, Voluta, Fasciolaria, Strombus* and *Murex*. The state of preservation, generally in that of casts, rendered it impossible to determine accurately the species. The limestone is traversed by two dykes of basalt, and it lies immediately under the Paul de Serra, a volcanic plateau which rises 2500 feet above the limestone.

The coal or lignite occurs on the north side of the island, on the banks of one of the tributaries of the St. George. Professor Johnston considers it to be the dried relict of an ancient peat bog, and its lustre, compactness and rhomboidal fracture to be due to the action of the basalt which overlies it. An analysis gave

Carbon	60·7
Hydrogen	5·82
Oxygen and nitrogen	33·47
	——99·99

and 20·05 per cent. of ash. This is the organic constitution of true peat; but no peat exists at present in Madeira, nor as far as Mr. Smith is aware, has any been noticed so near the equator. He therefore suggests that this deposit may indicate a former colder climate in that latitude.

The sands of Caniçal are found near the eastern extremity of the island, in a valley which extends from the northern to the southern shore; and they consist of small particles of basalt and comminuted testacea, enclosing vast numbers of land shells as well as calcareous incrustations of plants. The shells have been most carefully examined by the Rev. Mr. Lowe, and one sixth ascertained to belong to species not now found living in the island; the Caniçal sands therefore are assigned by Mr. Smith to the Pleistocene or newest tertiary æra. The calcareous incrustations have been considered by some observers not to be of vegetable origin, in consequence of the general absence of organic structure; and Dr. Macaulay is of opinion, from their consisting of carbonate and phosphate of lime, silica and animal matter, that they are of animal origin, and probably belonged to the family of *Alcyonidæ*. As however they are mere casts, Mr. Smith conceives that an analysis cannot throw light on their origin; and as all of them bear the most perfect resemblance to trunks or branches, and one of his specimens exhibits impressions externally of cellular structure, he has no doubt of their being the calcareous casts of plants.

In one of the small islets adjoining Porto Santo is a bed of fossiliferous limestone, which supplies the kilns of Funchal. The fossils consist almost exclusively of casts, but Professor Agassiz having been enabled to identify some of them with casts of recent species, Mr. Smith infers that the limestone is an extremely modern formation, though it has all the characters of primary marble. In this case, the volcanic action, Mr. Smith states, was evidently submarine, as the contact of the basalt and the limestone is so intimate that the two rocks never separate when a mass composed partly o each is detached by force. The elevation of the islet above the level of the sea has not, however, disturbed the horizontal position of the beds.

On the island of Porto Santo the volcanic action was sub-aërial, as the basalt is scoriaceous on the surface, and rests on volcanic brick. In this island there is a sandy deposit similar to that at Caniçal. The Disertas, lying about three leagues to the south-east of Madeira, Mr. Smith describes as a chain of volcanic mountains

ranging north and south, or nearly at right angles to that of Madeira. The sea-cliffs reach to their very summits, and exhibit a series of beds of basalt, ashes, tufas, and volcanic brick, intersected by innumerable dykes. No fossils have yet been discovered on these islands.

The occurrence of the marine limestone of San Vincente at an elevation of 2500 feet, proves a relative change in level of land and sea to that amount, previously, Mr. Smith is of opinion, to the ejection of the overlying volcanic products; but he has not observed in Madeira any proofs of elevation of the land during or subsequent to the volcanic period; though there are strong indications of subsidence, the beds of scoriæ and ashes, and those containing vegetable remains, dipping under the sea, and occurring in situations where they could not have remained, had the sea level been always the same as at present.

A letter, dated Madras, July 1840, addressed to John Taylor, Esq., Treas. G.S., by Mr. Frederick Burr, on the Geology of Aden, on the coast of Arabia, was afterwards read.

The promontory of Aden, eighty miles eastward of the Straits of Bab-el-mandeb, consists of a bold cluster of volcanic rocks with lofty jagged peaks, and is connected with the main land by a low isthmus. Its extreme length is about six miles, and its breadth is about three miles, and the summit of the highest point is about 1776 feet above the level of the sea. The loftier portions of the promontory are wholly volcanic, and the lower are partly volcanic and partly consolidated sea-sand. The most interesting portion of the district is an immense, nearly circular crater, situated at the extremity of the promontory next the main land, and in the centre of which, upon a flat little raised above the sea-level, stands the town of Aden. The diameter of the crater is about one and a half mile, and it is surrounded on all sides but the eastern with precipices chiefly composed of lava, and rising from 1000 to 1776 feet in height. Although the crater appears at first sight almost perfect, Mr. Burr says, it has been affected by some rude shocks which have cleft it entirely through from north to south, forming two rents, known as the northern and southern passes. The portion to the west of the fissures, and called the Gebel Shunsam, rising to the height of 1776 feet, stands entire; but that to the east has evidently undergone a partial subsidence, attaining to not more than half the height of the western side, and for the distance of about half a mile it has been broken down, allowing the sea to come almost close to the town and form a little bay; but the direction of the original outline of the crater is indicated by the island of Seerah, situated in about the middle of the gap.

To the northward of this great crater is an immense mass of lofty and jagged volcanic products, probably the remains of smaller craters.

The prevailing rock is a dark brown or chocolate lava, generally of a very cellular structure. About the middle of the east side of the great crater, it contains a very thick mass, composed of alterna-

tions of greenish porphyry, slightly lamellar in structure, and of red ochreous clay. Near the northern pass Mr. Burr noticed a granular rock, or volcanic breccia. The inclination of the beds is generally 15° from the crater.

Numerous perpendicular dykes intersect the volcanic rocks, and are harder and more compact than the beds they traverse. Small veins of calcedony also occur.

Dr. Malcolmson showed Mr. Burr some specimens of black and green obsidian obtained on the promontory, but the conditions under which they exist Mr. Burr was prevented from ascertaining.

The deposits of consolidated sea-sand occur more especially near the northern pass, towards the base of the volcanic ridges. The stratification is diagonal, and this arrangement Mr. Burr conceives to have been produced by the drifting of opposing currents. The flat line of coast on the northern part of the promontory, the author says, is evidently a raised beach, and the consolidation of the sand he assigns to the action of a tropical sun upon the calcareous materials. The stone incloses numerous shells and corals of species existing in the Arabian Sea.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1841.

Chiswick.—June 1, 2. Very fine. 3, 4. Fine, with very dry air. 5. Slightly overcast. 6. Very fine: slight rain. 7. Cold and dry: showery and cold. 8, 9. Cloudy and cold. 10. Very fine. 11. Cloudy. 12. Slight rain: clear. 13. Cloudy and cold: clear at night. 14. Very fine. 15. Slight drizzle: cloudy and fine. 16, 17. Very fine. 18. Sultry: rain, with distant thunder and lightning at night. 19. Sultry: rain. 20. Very fine: rain. 21. Heavy showers, with sultry intervals. 22. Very fine: cloudy. 23. Overcast and fine: very heavy rain at noon. 24. Showery: heavy rain at night. 25. Rain. 26. Cloudy. 27. Showery. 28. Rain. 29. Cloudy: showery. 30. Fine.—The mean temperature of the month was about 2° below the average.

Boston.—June 1—4. Fine. 5, 6. Cloudy. 7. Cloudy: rain A.M. and P.M. 8. Cloudy: rain early A.M. 9. Cloudy. 10. Fine. 11—13. Cloudy. 14—17. Fine. 18. Fine: therm. 3 o'clock 74°. 19. Cloudy: rain early A.M.: rain P.M. 20. Fine: rain P.M. 21. Cloudy: brisk wind. 22, 23. Fine. 24. Fine: rain, with thunder and lightning P.M. 25. Rain. 26. Cloudy: rain P.M. 27. Fine. 28. Rain: rain P.M. 29. Fine: rain with thunder and lightning P.M. 30. Fine.

Applegarth Manse, Dumfries-shire.—June 1. Clear and warm. 2. Bright and cool. 3. Hail-showers: thunder. 4. Cloudy. 5. Cloudy: rain P.M. 6. Fine but cloudy. 7. Dry and cool. 8. Dry and cool: withering. 9—11. Dry and cool. 12. Dry and cool, but warmer. 13. Dry and cool. 14. Slight showers. 15, 16. Dry and droughty. 17. Dry and droughty: cloudy. 18. Fine rain and thunder. 19. Rain P.M. 20. Very warm: rain P.M. 21. Heavy showers. 22. Fair all day. 23. Fair and fine. 24. Fair and fine: thunder. 25. Wet nearly all day. 26. Slight showers: thunder. 27. Fair till 4 o'clock: rain. 28. Showery all day. 29. Showery all day: thunder. 30. Rain P.M.

Sun shone out 28 days. Rain fell 11 days. Thunder 4 days. Hail 1 day.

Wind north 2 days. North-east 4 days. East-north-east 1 day. East $3\frac{1}{2}$ days. South-east 2 days. South-south-east 2 days. South $3\frac{1}{2}$ days. South-south-west 1 day. South-west 4 days. West $4\frac{1}{2}$ days. West-north-west 1 day. North-west $1\frac{1}{2}$ day.

Calm 7 days. Moderate 9 days. Brisk 2 days. Strong breeze 9 days. Boisterous 3 days.

Mean temperature of the month	54°·05
Mean temperature of June 1840	54 ·40
Mean temperature of spring-water	50 ·80

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. June.	Barometer.						Thermometer.									Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·214	30·137	30·122	29·50	29·98	30·00	64·3	80·5	56·2	72	47	65	62	51	S.	NE.	W.	W.	...	...	...	...	61
2.	30·272	30·177	30·167	29·55	29·98	30·08	65·0	82·3	55·3	75	53	66	56	41½	S.	W.	W.	W.	...	...	...	...	62
3.	30·250	30·254	30·157	29·62	29·98	30·05	63·0	73·4	56·5	73	41	63	55	38½	N.	W.	W.	SW.	...	...	...	...	59
○ 4.	30·440	30·357	30·300	29·79	30·15	30·11	62·7	73·6	52·2	72	46	57	56	47½	W.	NW.	NW.	SW.	...	...	...	...	53
5.	30·250	30·210	30·004	29·65	30·00	29·92	62·2	73·6	52·9	73	46	58	58	48	W.	W.	W.	SW.	...	·03	...	...	54
6.	30·036	29·981	29·923	29·55	29·95	29·92	55·3	69·0	51·3	59	40	51·5	61	40½	WNW.	NE.	NW.	N.	·019	·01	...	...	54
7.	29·964	29·956	29·921	29·40	30·00	30·04	51·3	60·0	45·3	57	46	49	56½	41	NW.	N.	NW.	N.	...	·02	...	0·06	50
8.	29·996	29·974	29·958	29·48	30·09	30·04	51·0	56·4	47·6	56	46	51·5	59½	43½	NW.	N.	N.	NE.	...	...	·25	...	51
9.	29·970	29·947	29·911	29·47	30·01	29·95	50·3	57·2	47·4	57	39	51·5	61	44	NNW.	NE.	N.	NE.	...	...	...	...	47
10.	29·838	29·815	29·669	29·27	29·87	29·69	52·3	57·6	47·8	71	41	57	76	50	NW.	N.	N.	NW.	...	...	...	...	47
11.	29·722	29·775	29·687	29·22	29·74	29·84	50·5	68·7	49·3	54	45	50	59	46½	NW.	N.	N.	NE.	...	...	...	...	50
☾ 12.	29·850	29·973	29·807	29·37	29·89	29·88	50·7	54·7	46·8	53	37	50	63½	41½	N.	NE.	N.	SW.	...	·01	...	...	47
13.	30·064	30·040	29·944	29·53	30·00	29·96	50·2	55·2	47·0	65	37	57	61	38	NW.	NW.	calm	NW.	...	...	...	...	47
14.	30·100	30·047	29·906	29·47	29·93	29·78	57·3	72·6	46·7	72	53	59·5	54	39	W.	NW.	calm	W.	...	...	...	0·04	48
15.	29·940	30·097	29·895	29·25	29·81	30·00	57·4	66·0	55·6	69	36	57	61	47½	W.	NW.	W.	W & E.	...	·01	...	...	53
16.	30·274	30·208	30·111	29·65	30·02	29·97	60·5	67·8	47·7	72	41	59	61½	44	S.	S.	calm	SW&S.	...	...	...	...	50
17.	30·136	30·079	29·875	29·39	29·91	29·83	61·7	80·3	51·5	70	41	66	61	52½	S.	S.	calm	S.	...	...	...	...	56
18.	29·822	29·788	29·605	29·18	29·70	29·61	64·3	76·4	53·6	80	54	65	59½	53½	NNW.	S.	calm	SE.	...	·24	...	...	56
● 19.	29·650	29·689	29·607	29·05	29·58	29·58	60·7	75·0	58·2	69	45	60	63	51½	W.	W.	calm	E.	·111	·08	·38	...	59
20.	29·842	29·759	29·724	29·22	29·58	29·40	64·3	87·0	51·2	66	54	63	65½	52	S.	SW.	calm	E.	·027	·02	·08	...	57
21.	29·848	29·959	29·769	29·18	29·42	29·62	64·2	71·0	56·3	71	47	65	61	52½	S var.	SW.	W.	SSE.	...	·26	·06	0·88	57
22.	30·118	30·059	29·994	29·50	29·78	29·80	61·7	70·2	53·7	73	46	64	61	49½	S.	W.	W.	SSE.	·366	·01	...	...	56
23.	30·030	30·028	29·812	29·42	29·81	29·75	62·0	69·7	53·4	73	46	64	64	50	S.	S.	calm	SSW.	...	·40	...	...	56
24.	29·800	29·741	29·639	29·25	29·70	29·68	63·0	69·4	53·8	72	54	66	67	46	E.	W.	calm	SE.	...	·49	...	...	55
☽ 25.	29·542	29·543	29·494	29·10	29·61	29·50	55·7	68·4	56·2	70	54	57	54	47	E.	SW.	E.	E&SE.	·525	·04	1·08	...	58
26.	29·686	29·774	29·605	29·10	29·43	29·51	64·0	78·0	56·2	67	52	64	66	48½	S.	SW.	S.	S.	·133	...	·32	...	59
27.	30·012	30·128	29·951	29·38	29·73	29·89	62·5	67·6	56·4	70	52	67	64	49½	W.	W.	W.	WNW.	...	·10	·34	...	59
28.	30·012	29·981	29·773	29·44	29·65	29·60	58·3	68·7	55·3	62	50	58	57	51	S.	S.	W.	S.	·430	·65	·01	1·00	58
29.	29·820	29·817	29·743	29·22	29·58	29·70	61·3	67·4	52·8	69	48	63	64	47½	S.	SW.	W.	E.	·675	·07	·30	...	57
30.	30·046	30·093	29·927	29·43	29·88	29·86	59·7	73·6	52·7	67	52	63	60	50½	W.	NW.	calm	W.	·022	·01	·23	...	56
Mean.	29·985	29·979	29·866	29·38	29·825	29·818	58·9	69·7	52·2	67·30	49·56	59·5	61	47·4					Sum. 2·308	2·45	3·05	1·98	Mean. 54

THE

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[THIRD SERIES.]

SEPTEMBER 1841.

XXVII. *On the Form and Optical Constants of Anhydrite. By* W. H. Miller, *M.A., F.R.S., Professor of Mineralogy in the University of Cambridge**.

THE following values of the angles between normals to the faces of anhydrite were obtained from the measurements of a single crystal,—the only one exhibiting any forms, except those of cleavage, which I have been able to procure. The faces being extremely dull, the only signal that could be used in observing with the reflective goniometer was a small opening in a screen, through which sun-light was thrown by reflexion from a plane mirror. The results obtained under such circumstances cannot be very accurate; they are, however, certainly much nearer the truth than those which Haüy has given, and which appear to have been adopted by all succeeding mineralogical writers.

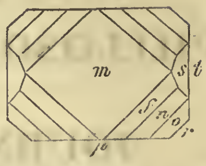
The most perfect cleavages are parallel to the faces m, t; a cleavage a little less perfect than the preceding exists parallel to the face p, which is usually rough; and cleavages still less perfect may be obtained parallel to the faces r. The face s is common to the zones $m\ t$, $o\ p$.

$m\ t$	90°	0′	$t\ f$	72°	56′
$t\ p$	90	0	$p\ o$	51	51
$m\ p$	90	0	$p\ n$	63	43
$p\ s$	90	0	$p\ f$	70	47
$t\ r$	48	18	$m\ o$	55	50
$r\ p$	41	42	$m\ n$	36	23
$t\ o$	56	36	$m\ f$	26	10
$t\ n$	66	45	$m\ s$	44	25

* Communicated by the Author.

The symbols of the simple forms are,—

t	{100},	r	{110},
p	{010},	o	{111},
m	{001},	n	{112},
s	{101},	f	{113}.



The optic axes lie in a plane parallel to the face m, and make angles of 21° 46′ with a normal to p. The ratios of the velocity of light in air to its velocity within the crystal are 1·571, 1·576, 1·614 for rays in planes parallel to t, m, p, polarized in those planes respectively.

St. John's College, July 30, 1841. W. H. MILLER.

XXVIII. *Abstract of recent Researches on the quantity of Heat evolved in Chemical Combination, particularly those of* MM. Dulong, Hess *and* Andrews.

[Continued from p. 25, and concluded.]

THE law described at the conclusion of the former portion of this abstract was founded on the following evidence, the apparatus used by M. Hess being the same employed in the experiments by which the first law was found. Sulphuric acid of various degrees of concentration, but of definite atomic constitution, was mixed with a slight excess of water of ammonia (sp. gr. 0·935), and the following results obtained:—

Oil of vitriol		$HO + SO_3$	gave	595·8	units of caloric.
Acid of	...	$2HO + SO_3$	...	518·9	...
...	...	$3HO + SO_3$	...	480·5	...
...	...	$6HO + SO_3$	...	446·2	...

As the experiment was not made with anhydrous acid, it need not be taken into account, and therefore commencing with oil of vitriol, and adding to the quantity of heat evolved by the ammonia in combining with each dilute acid, the quantity of heat which had been previously evolved by the oil of vitriol, therein, to form the dilute acid, the following numbers are obtained:—

Acid, of strength	Had evolved in combining with Water.	Evolves in combining with Ammonia.	Total.
$HO + SO_3$	.....	595·8	595·8
$2HO + SO_3$	77·8	518·9	596·7
$3HO + SO_3$	116·7	480·5	597·2
$6HO + SO_3$	155·6	446·2	601·8

giving a mean of 597·9. But since real sulphuric acid evolves in combining with water 510·1 units of heat, and since acid of $6\,HO + SO_3$ would still evolve with water 38·9, the total quantity of heat which would have been evolved by the mixture of dry sulphuric acid with water of ammonia would be $510·1 - 38·9 + 597·9 = 1069·1$.

The second series of experiments was executed with potash, and gave the following results. M. Hess was obliged, however, to modify the mode of working. In consequence of potash combining with water in various proportions, and evolving thereby different quantities of heat, it was necessary to use the solution of potash so weak as not to be affected by any further addition of water; and the mass of materials became then so large as to preclude the use of the calorimeter, which had hitherto served so well. The method of mixtures was therefore adopted. The vessel in which the mixture was effected was nearly globular in form, and about a litre in capacity; it weighed 290 grammes, equivalent to fifty-five grammes of water. The water was first introduced, and then so much solution of potash as was necessary for neutralizing a certain quantity of acid. The temperature of the liquid was observed immediately on the addition of the potash, and the rise of temperature on the addition of the acid. All these fluids were measured, and when the temperature had fallen to the standard, the specific gravity was taken which gave the weight. The specific heat of the solution was found to be 0·943.

Sulphuric acid	$6\,HO + SO_3$	gave	445·4	units of heat.
...	$3\,HO + SO_3$	...	483·4	...
...	$2\,HO + SO_3$	...	527·1	...
...	$HO,\ SO_3$	...	597·2	...

By establishing the same comparison here as in the case of ammonia, we find that

Acid, of strength	Had evolved in combining with Water.	Evolves in combining with Potash.	Total.
$HO + SO_3$	.....	597·2	597·2
$2\,HO + SO_3$	77·8	527·1	604·9
$3\,HO + SO_3$	116·7	483·4	600·1
$6\,HO + SO_3$	155·6	445·4	601·0

giving a mean of 601·0.

The experiments with soda were carried on in the calorimeter, and gave with $HO + SO_3$, 608·7 units of heat; and with $6\,HO + SO_3$, 447·4 units. But $447·4 + 155·6 = 603·0$.

M. Hess remarks that, as these numbers are sensibly the same as those obtained for potash, the rise of temperature, which occurs when $Na\,O + S\,O_3$ is mixed with $10\,H\,O$, does not take place when the solutions are mixed together; and hence infers, that the $Na\,O$ is already combined with at least 10 Aq.

For carrying on the experiments with lime, some additional circumstances required to be observed. The lime was weighed dry, and then put into a bottle with 700 grammes of water, when it slaked and evolved heat. After having waited until the temperature had fallen to that of the surrounding air, the acid was added. In this, as in all other experiments of this kind, the alkaline substance was left slightly in excess. After mixture the elements are evidently differently arranged from before, and consequently M. Hess always determined the specific heat of the mixture by a direct experiment when it was liquid; but as this could not be done in the case of lime, it was necessary to determine the composition of the mixture, and the specific heat of each of its ingredients.

The numerical results obtained were,—

Sulphuric acid	$6\,H\,O + S\,O_3$	gave	481·8	units of heat.
...	$2\,H\,O + S\,O_3$	...	543·5	...
...	$H\,O + S\,O_3$	...	628·3	...

Adding, as before, the heat evolved by the previous dilution with water, we obtain with

$$H\,O + S\,O_3 \;\ldots\qquad 628{\cdot}3$$
$$2\,H\,O + S\,O_3 \;\ldots\; 77{\cdot}8 + 543{\cdot}5 = 621{\cdot}3$$
$$6\,H\,O + S\,O_3 \;\ldots\; 155{\cdot}6 + 481{\cdot}8 = 637{\cdot}4$$

These experiments were repeated in the calorimeter, and the results were,—

Sulphuric acid	$6\,H\,O + S\,O_3$	gave	489·2	units of heat.
Second experiment, same acid		...	490·9	...
Acid of	$2\,H\,O + S\,O_3$	...	564·6	...
Second experiment, same		...	556·4	...
Acid of	$H\,O + S\,O_3$	...	645·0	...

Adding thereto the heat of dilution, we have,

$H\,O + S\,O_3$	....	$645 + 0 = 645$
$2\,H\,O + S\,O_3$	....	$564{\cdot}6 + 77{\cdot}8 = 642{\cdot}4$
Ditto	....	$556{\cdot}4 + 77{\cdot}8 = 634{\cdot}2$
$6\,H\,O + S\,O_3$	....	$490{\cdot}9 + 155{\cdot}6 = 646{\cdot}5$
Ditto	....	$489{\cdot}2 + 155{\cdot}6 = 644{\cdot}8$

The mean of these is 642·6. The mean of the former series was 628·9, which, from a number of sources of loss, is necessarily too small. But if we take the higher number, it may

be asked how lime differs so much from the other bases, which give about 600. This may depend upon two things; *first*, that the two atoms of water, which gypsum contains, evolve heat, or else it could not be a chemical combination; or, *secondly*, that it may arise from the solidification of the sulphuric acid. In order to obtain some indications on this point, M. Hess made two experiments on the quantity of heat evolved by gypsum in setting, and found that for $S\,O_3$ there are evolved 36·5 to 37·7 units of heat.

If we subtract that from the 642·5, we have 604·8 for the heat evolved with oil of vitriol, or 1076·1 with anhydrous sulphuric acid.

M. Hess also ascertained that dry lime, in combining with water, evolves 167·2 units of heat.

Experiments with Muriatic Acid.

M. Hess employed the acid $H\,Cl + 12\,H\,O$, which contains 0·252 of dry acid, and has sp. gr. 1·125. This acid still evolves heat when mixed with water, and the quantity being determined, as in the case of sulphuric acid, was found to be 50·84 units of heat for one atom of muriatic acid. In calculating this result, however, the atom of sulphuric acid is still taken as unit, which makes the atom of muriatic acid 0·908.

The acid $H\,Cl + 12\,H\,O$ (previously diluted) being saturated with ammonia, gave out 318·8 units of heat; and in another trial exactly the same number was obtained.

Three experiments were made by means of the acid $H\,Cl + 12\,H\,O$, the water and the ammonia being all mixed at the same time; the heat evolved was 367·7, 364·9, and 374·7, giving a mean of 369·1. If we then take the former result, we have $318{\cdot}8 + 50{\cdot}84 = 369{\cdot}64$.

The acid $H\,Cl + 12\,H\,O$, mixed with water and potash, evolved 361·9 units of heat.

The acid $H\,Cl + 12\,H\,O$, mixed with water and soda, evolved 376·4 units in one and 360· units in another experiment.

It has been already remarked, that the general principle of the quantity of heat evolved in combustion being proportional to the quantity of oxygen absorbed, was supported, at least approximately, by the example of carbon, in forming carbonic oxide and carbonic acid. This subject has, however, given rise to some difference of interpretation of Dulong's number, given in page 20. M. Hess, who considers that carbonic oxide contains its own volume of gaseous carbon, deduced therefrom, that the quantity of heat evolved in forming car-

bonic oxide, was, to that evolved in burning carbonic oxide, as three to two. As, according to his view,

A vol. of carbon burning to carbonic acid evolves heat	7858
A vol. of carbonic oxide burning evolves heat . . .	3130
∴ A vol. of carbon forming a vol. of carbonic oxide evolves	4728

From this he deduced the curious result, that it would produce more heat in practice (as in smelting-works) to employ all oxygen to form carbonic oxide, than to produce carbonic acid in the proportion of six to five; and hence, that in changing carbonic acid into carbonic oxide, heat should be evolved. These views have, however, been shown to be erroneous by M. Ebelmen, who proved, by a comparison of Despretz's results with those of Dulong, that Dulong considered carbonic oxide to contain half its volume of gaseous carbon, and, consequently, that as

A vol. of carbon, burning to carbonic acid, evolves	7858 heat,
Two vols. of carbonic oxide, burning, evolves . .	6260
One vol. of carbon, in forming carbonic oxide, evolves only	1598

and hence the quantity thus employed is, to the total quantity, as one to four.

M. Hess has very ingeniously endeavoured to apply his second law to the determination of several important questions. Thus, to explain the great combustibility of gunpowder, he considers that in forming nitric acid, each atom of oxygen has evolved a certain quantity of its heat, and more in the order of its combination. Thus, if the total quantity of heat belonging to an atom of oxygen be represented by 16, then the first atom which unites with the nitrogen may be supposed to lose all; the second 8, the third 4, the fourth 2, and the fifth 1. There is thus expelled in forming nitric acid, $16 + 8 + 4 + 2 + 1 = 31$ units of heat; but the total quantity belonging to the oxygen being $16 \times 5 = 80$, there remain 49 units of heat available for the carbon and sulphur. A very important consequence from these considerations, deduced by M. Hess, is, that the heat evolved in the combustion of a compound body is, in all cases, less than the heat evolved in the combustion of its constituents; because some heat has been already eliminated in the combination of these constituents with each other. This principle M. Hess considers to derive great support from experiments made by Dr. Ure on the combustion of certain kinds of fuel and fatty matters, of which there is a notice in the Athenæum, 1839, No. 620; but it must be remarked, that the re-

sults of Dulong, as given in page 22, with Berzelius's remarks, are directly opposed to this idea.

In connexion with the subject of this abstract we have to notice a paper by Dr. Andrews of Belfast, 'On the Heat developed during the Combination of Acids and Bases,' read before the Royal Irish Academy, January 11, 1841*.

The general conclusions at which the author arrives are contained in the two following laws:—

Law 1. The heat developed during the union of acids and bases is determined by the base, and not by the acid; the same base producing, when combined with an equivalent of different acids, nearly the same quantity of heat, but different bases a different quantity.

Law 2. When a neutral is converted into an acid salt, no change of temperature occurs.

In the commencement of the paper a preliminary experiment is described, the object of which is to determine the exact quantity of heat evolved during the combination of nitric acid and potash. The solutions, both acid and alkaline, were taken so weak in this and all the other experiments detailed in the communication, that subsequent dilution with water did not produce any change of temperature. On neutralizing the solution of caustic potash, containing 0·353 gramme of pure alkali, with nitric acid, the temperature of the resulting solution of nitrate of potash, whose weight amounted to 30 gr., was found (after all corrections had been made) to rise 6·75° F.

To illustrate law first, the author adduces tables, which show, at a glance, the heat produced when an equivalent of each base is neutralized by different acids. Thus, when the same proportion of pure potash is combined under similar circumstances with the arsenic, phosphoric, nitric, boracic, hydrochloric, hydriodic and oxalic acids, the elevations of temperature, indicated by the thermometer, vary only from 6·8° to 6·6°. Sulphuric acid produces rather a higher temperature than any other acid (7·3°), and the acetic, formic, tartaric, citric, and succinic acids, give rather less heat than those before mentioned (from 6·4° to 6·1°). In like manner, ammonia produces an increase of temperature varying from 5·7° to 5·5°, when neutralized by the nitric, hydrochloric, hydriodic, arsenic, oxalic, and acetic acids; the greatest divergence from these numbers occurring, on the one hand, with the sulphuric acid (6·3°), and on the other, with the citric, tartaric, and succinic acids (5·1°). Analogous results are described as having

* From the Proceedings of the Royal Irish Academy: the paper will appear in vol. xix. Part II. of the Transactions of the Academy.

been obtained with other bases, such as soda, barytes, magnesia, lime, and the oxides of zinc and lead. On the contrary, the heat developed by each base is peculiar to itself; and, consequently, the same acid gives different elevations of temperature, with equivalents of different bases. To take, as an example, the nitric acid, which also produces very nearly the mean quantity of heat given by all the acids, the following numbers express the increments of temperature obtained on combining the same quantity of it with each base:—magnesia, 8·1°; lime, 7·2°; barytes, 6·9°; potash, 6·8°; soda, 6·5°; ammonia, 5·6°; oxide of zinc, 4·8°; oxide of lead, 4·2°; oxide of silver, 3·2°. The numbers for barytes, potash, soda and ammonia, are strictly comparable with one another (except a slight correction for differences in the specific heats of the solutions); but in the case of the other bases, an absorption of heat, unknown in amount, takes place in consequence of their conversion from the solid to the fluid state. Hence the numbers for these bases are all below the truth.

Two singular anomalies are described as occurring in the combinations of the peroxide of mercury with the hydracids, and in those of the hydrocyanic acid with the bases.

In confirmation of the second law, the author adduces a series of experiments, which prove, that during the conversion of a neutral into a supersalt no heat is produced. Thus while the normal development of heat occurs when a solution of caustic potash is neutralized by oxalic acid, the subsequent additions, first of one, and afterwards of two more atoms of the same acid, so as to convert the neutral oxalate into the binoxalate, and the latter again into the quadroxalate of potash, are not accompanied by any change of temperature in the solutions. In testing the accuracy of this law, it is necessary to select examples where all the compounds are soluble in water, otherwise the heat arising from the formation of precipitates would interfere with and complicate the result.

The second law does not extend to the case of the conversion of neutral into basic compounds,—a part of the subject which the author has carefully investigated.

XXIX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 49.]

Preparation of Urea.

THE usual method of preparing urea by precipitating it from urine by means of nitric acid, is attended with great expense of time and material. Prof. Liebig has recently published the

following new and easy method :—Twenty-eight parts of perfectly dried ferrocyanate of potash are mixed with fourteen parts of the black oxide of manganese, both finely powdered, the mixture heated on a smooth iron plate, (not in a crucible) over a coal fire, to incipient redness, stirring frequently. The mass on cooling is washed out with cold water, and the liquid mixed with twenty and a half parts of dry sulphate of ammonia. It is advantageous to place aside the first strong ley obtained, and to dissolve the sulphate of ammonia at the common temperature in the last water, and to add this to the first concentrated solution. Usually a strong precipitate of sulphate of potash is immediately formed, from which the liquid is separated by decantation; it is then evaporated in the water bath or on some warm place, boiling being avoided, and the liquid poured off, as long as it is possible, from the deposit of sulphate of potash. It is then evaporated to dryness and treated with boiling alcohol of 80-90 per cent., which dissolves the urea, while the sulphate salts remain undissolved. In this manner nearly four ounces of perfectly colourless and beautifully crystallized urea may be obtained from a pound of the ferrocyanate of potash. It frequently happens that the solution containing the sulphate of potash and urea is coloured yellow by ferrocyanide of ammonium, or of potassium, which dissolves in the alcohol, and gives to the crystals of urea a yellowish colour; it may easily be separated by the addition of a solution of copperas, after separation of the Prussian blue carbonate of ammonia is added to the liquid, which decomposes the excess of iron salt, and the liquid becomes clear and colourless, and may then be evaporated and treated as above. (*Ann. de Chem. und Pharm.* vol. xxxviii. part 1.)

Arseniuretted Hydrogen.

It is well known that a solution of the perchloride of mercury most easily decomposes arseniuretted hydrogen, on which account it is employed not only to destroy every trace of this gas, but likewise to indicate its presence. The composition of this precipitate, which is yellow with a slight brownish tinge, and is thus distinguished from the precipitate produced by the action of perchloride of mercury on phosphuretted hydrogen, is quite unknown; Stromeyer seems to be the only person who has examined it. According to him, arseniuretted hydrogen forms, with a solution of perchloride of mercury, arsenious acid and protochloride of mercury, and, finally, an amalgam of mercury and arsenic. The precipitate decomposes by preservation in much water; it becomes black, and consists at last only of globules of mercury; the liquid above it contains hydro-

chloric and arsenious acids. This decomposition is perfectly similar to that which is caused by water in the precipitate produced in solutions of the perchloride of mercury by phosphuretted hydrogen, only that it is effected more rapidly. The similar decomposition of both precipitates by water, and also by dilute nitric acid, supposes a similarity in their constitution, which was confirmed by a quantitative analysis, according to which the precipitate proved to be composed according to the formula $As^2 Hg^3 + 3\, Hg\, \underline{Cl}$. This precipitate differs from that produced by phosphuretted hydrogen, by its being anhydrous, while the latter contains three atoms of water. This is the reason of their different action at a high temperature. The precipitate produced by antimoniuretted hydrogen in solutions of the perchloride of mercury, has quite a different composition to that produced by phosphuretted or arseniuretted hydrogen in such solutions. Whence we may conclude that the composition of antimoniuretted hydrogen differs from those of phosphuretted and arseniuretted hydrogen. (H. Rose in *Pogg. Ann.* li. p. 423.)

On the Salts of Lead formed by Nitrous Acid and Hyponitrous Acid.

Proust was the first to observe that lead dissolves in considerable quantity when brought into a hot solution of the nitrate of lead; the salt thus produced is deposited in the form of yellow lustrous scales. Proust concluded from this experiment that the oxide of lead was reduced to a lower degree of oxidation; however, Berzelius subsequently showed that the solution of the lead was effected at the expense of the nitric acid in the salt employed. About the same time Chevreul described two different salts produced by the action of different quantities of lead on the nitrate of lead, and arrived at nearly the same conclusions as Berzelius. Péligot has recently shown that three very distinct salts are formed by the action of lead on the nitrate of lead, two of which contain not nitrous acid*, as supposed by Berzelius and Chevreul, but hyponitric acid†; accordingly the latter acid, which, according to Dulong's analyses, consists of two volumes of nitrogen and four of oxygen, would, contrary to the general opinion, be able, if not to combine directly with bases, yet at least to exist in combination with them. Proust's salt is best prepared by bringing together one equivalent of lead and one equivalent of the nitrate; the reaction proceeds to the last at a temperature below 60° or 70°, without any evolution of ni-

* Hyponitrous acid of the English chemists.

† Nitrous acid of the English chemists.

tric oxide; this gas escapes only when the yellow salt is decomposed at a higher temperature. Should this salt be mixed with any of the orange-coloured salt which will presently be noticed, it may, from its far greater solubility, be separated from it by treating the mixture with a quantity of warm water, not sufficient to dissolve the whole. The analysis consisted in the direct determination of the oxide of lead, nitrogen and water; the results agree with the formula $\underline{N}\,O^4$, $2\,Pb\,O$, $\underline{H}\,O$. Berzelius adopts hypothetically that it contains 6·4 per cent. water; experiment affords but 3·2. The following simple equation, $\underline{N}\,O^5, Pb\,O + Pb + \underline{H}\,O = \underline{N}\,O^4, 2\,Pb\,O, \underline{H}\,O$, gives good account of the origin of the salt. The second salt is of an orange colour, and is obtained by dissolving two and a half equivalents of lead in the boiling solution of one equivalent of the nitrate of lead; on cooling, a mixture of the yellow and orange salts is obtained; the former is removed with boiling water. All the analyses of this salt give the formula $\underline{N}\,2\,O\,8$, $7Pb\,O$, $3\,\underline{H}\,O$. The constitution of this salt is confirmed by synthesis, for when the bibasic hyponitrate is boiled with oxide of lead, the orange-coloured salt is obtained. Continued boiling of nitrate of lead with more than two or three equivalents of lead gives Chevreul's rose-red salt, which is represented by the formula $\underline{N}\,O_3$, $4\,Pb\,O$, $\underline{H}\,O$. (*Compt. Rend.* t. xi. p. 860.)

Deville *on Oil of Turpentine.*

M. Deville calls the oil which is contained in the artificial camphor Camphen, that in the fluid product Tereben. Tereben may be obtained by distilling oil of turpentine with sulphuric acid. The temperature must not exceed 200°, for then colophen passes over. By repeated distillation with sulphuric acid, the tereben may be obtained pure; the only proof of its purity is its having lost all power of rotation. By distilling the liquid camphor with lime it cannot be obtained pure. It has the same boiling point and specific gravity as oil of turpentine. Formula $C^{20}H^{32}$. Tereben combines with the hydrochloric, hydriodic and hydrobromic acids in two proportions. The monohydrochlorate of tereben is $C^{20}H^{32} + H\,Cl$; the bihydrochlorate is $C^{20}H^{32} + H^2Cl^2$. This latter was examined by Soubeiran and Capitaine; it is the liquid product obtained in preparing artificial camphor; it has probably no power of rotation. The monohydrochlorate is obtained by passing hydrochloric acid into tereben; it is liquid; specific gravity at 20° = 0·902; smells like camphor; contains four volumes of vapour of tereben, and two of hydrochloric acid.

Power of rotation $= 0$, which is the case with all the other compounds of this class. The monohydrobromate is a colourless fluid; specific gravity $= 1{\cdot}021$ at 24°; obtained by acting on tereben with hydrobromic acid, and purifying by means of chalk, animal charcoal and chloride of calcium.

The bihydrobromate is obtained with the camphen compound by acting on oil of turpentine with hydrobromic acid; it is difficult to procure in a pure state; its power of rotation could not, therefore, be accurately determined. The monohydriodate is obtained in the same manner. Sp. gr. $= 1{\cdot}084$ at 21°; decomposes by exposure to the air. The bihydriodate can only be obtained mixed with the camphen compound.

By the action of chlorine and bromine on tereben, chlorides and bromides are produced. The specific gravities are respectively, 1·360 and 1·978 at 20°. Formulæ $C^{20}\,H^{24}\,Cl^{8}$, $C^{20}\,H^{24}\,Br^{8}$; eight atoms of hydrogen have been replaced by eight atoms of chlorine or bromine. By distilling these compounds two others may be obtained, $C^{20}\,H^{28}\,Cl^{4}$, or $C^{20}\,H^{28}\,Br^{4}$. The monochloride of tereben, $C^{20}\,H^{28}\,Cl^{4}$, has a specific gravity $= 1{\cdot}137$ at 20°.

The action of iodine appears to be more complicated.

Hydrobromate and hydriodate of camphen may be obtained in the same manner as the hydrochlorate; the former is solid; power of rotation $= -0{\cdot}4264$. The hydriodate is a fluid. Sp. gr. at $15° = 1{\cdot}0597$; power of rotation $= -0{\cdot}1597$. They both decompose by exposure to the air. Formulæ $C^{20}\,H^{32} + H^{2}\,Br^{2}$, and $C^{20}\,H^{32} + H^{2}\,I^{2}$.

By the action of chlorine on hydrochlorate of camphen, a fluid body is first produced, the formula of which is $C^{20}\,H^{24}\,Cl^{8}$, $Cl^{2}\,H^{2}$; this body then loses hydrochloric acid and forms a solid crystallized substance, chloride of camphen, $C^{20}\,H^{24}\,Cl^{8}$. Chloride of camphen has no power of rotation; sp. gr. at $8° = 1{\cdot}50$; melts at 110—115°. By its distillation another chloride is formed, the formula of which is probably $C^{20}\,H^{28}\,Cl^{4}$.

Oil of turpentine is decomposed by chlorine and bromine, and forms two bodies, whose formulæ are $C^{20}\,H^{24}\,Cl^{8}$, and $C^{20}\,H^{24}\,Br^{8}$. The power of rotation of the chlorine compound is (for the yellow ray) $+ 0{\cdot}2854$.

Colophen is formed, as above stated, by the action of sulphuric acid on oil of turpentine; it passes over at a temperature above 200°. Formula $C^{20}\,H^{32}$; specific gravity at $9° = 0{\cdot}940$; boils at 310—315°; sp. gr. of its vapour is 11·13.

Colophen is also formed by the rapid distillation of colophonium, but is always impure. Deville supposes that Unverdorben's pinic and sylvic acids are respectively oxides of colophen and tereben; for, by the distillation of colophonium,

a product is also obtained which is probably tereben, but mixed with a large quantity of colophen. Chloride of colophen is crystallizable. Formula is probably $C^{40} H^{64} Cl^{8}$. Colophen also combines with hydrochloric acid, but the compound is decomposed by treatment with carbonate of lime, &c. &c.—(*Annales de Chimie et de Phys.*, t. lxxv. p. 37.)

On the Æthereal Oils. By Gerhardt *and* Cahours.

Æthereal oils are mixtures of two substances, a hydrocarbon and a peculiar oil containing oxygen. The hydrocarbon may be driven off by distillation, but cannot thus be obtained pure; this is only effected by means of fused potassa. By the action of this substance on the oil, the hydrocarbon is separated, and the peculiar oil, which was combined with the hydrocarbon, is converted into an acid. By thus treating Roman caraway oil (*Cuminum Cyminum*), cuminic acid is obtained; from oil of valerian (*Valeriana officinalis*) and Roman oil of camomile (*Anthemis nobilis*), valerianic acid is obtained. The hydrocarbons contained in these two last-mentioned oils are different; that from oil of valerian is true camphogen, and oxidizes in the air with great rapidity, forming camphor. The hydrocarbon from caraway oil is called cymen; the body, which is changed into an acid by means of potassa, is cuminol.

Cuminol may be obtained by distilling oil of caraway at a temperature of 200°; cymen mixed with cuminol passes over, and cuminol remains behind, which may then be distilled in an atmosphere of carbonic acid. Its formula is $C^{20} H^{24} O^{2}$; sp. gr. of the vapour 5·24, according to calculation 5·094. It is colourless, boils at 220°, absorbs oxygen and becomes acid; this change takes place more rapidly when a base is present to saturate the so-formed acid. By means of bichromate of potassa and sulphuric acid, it is also converted into cuminic acid. Aqueous chlorine acts in the same manner, also strong nitric acid. Cuminic acid is best obtained by dropping the oil into fused potassa, the mass is then dissolved in water and precipitated by nitric acid. Cuminic acid is white, crystallizable, sublimes in beautiful needles, melts at 92°, boils at 250°; formula $C^{20} H^{24} O^{4}$. By the action of potassa on cuminol, two atoms of water are decomposed, hydrogen is set free, and two atoms of oxygen taken up for the formation of cuminic acid; almost insoluble in cold water; somewhat soluble in dilute acids; easily soluble in alcohol and æther. The silver salt has the formula $C^{20} H^{22} O^{3}, Ag O$. By heating this salt a carburet of silver, $Ag C$, is obtained. Cuminic æther is obtained by saturating an alcoholic solution of cuminic acid with hydrochloric acid, heating and distilling over oxide of lead.

It is a colourless liquid, lighter than water, boils at 240°; insoluble in water, soluble in alcohol and æther. Formula $C^{20} H^{22} O^3$, $C^4 H^{10} O$. Specific gravity of vapour = 6·65, calculated 6·583.

By the action of potassa on cuminol, without the aid of heat, a peculiar gelatinous substance is formed—cuminol-potassium (*potassio-cuminol*), $C^{20} H^{22} K O^2$. This is decomposed by water into cuminol and potassa; it absorbs oxygen and forms cuminate of potassa. Chloride of cuminol is obtained by acting with chlorine or cuminol in the sunlight. The chloride is a colourless fluid, but decomposes with great rapidity. Formula $C^{20} H^{22} Cl^2 O^2$. Forms, by boiling with potassa, cuminate of potassa and chloride of potassium; by exposure to a moist atmosphere it also forms cuminic acid. An amide compound does not appear to exist. The action of bromine is exactly the same as that of chlorine. By distilling one part of cuminic acid with four parts of caustic baryta, a colourless oil is obtained, and carbonate of baryta remains in the retort. This oil is *cumen*; it is colourless; smells like benzin; boils at 144°; formula, $C^{18} H^{24}$; sp. gr. of the vapour = 3·96, calculated 4·12. Cumen, $C^{18} H^{24}$, is cuminic acid, minus carbonic acid, $C^{20} H^{24} O^4 - C^2 O^4$. Cumen is insoluble in water; soluble in alcohol and æther; forms, with anhydrous sulphuric acid, a peculiar compound—cumensulphuric acid. The baryta salt crystallizes in beautiful lamellæ. Formula $C^{18} H^{22}, S^2 O^5 + Ba O$. Retinyl (*Retinylène*) has the same formula as cumen; it boils, however, at 150°; it combines in the same manner with sulphuric acid. The baryta salt, however, forms only crystalline crusts. Formula is the same, viz. $C^{18} H^{22}, S^2 O^5 + Ba O$. It also differs from the cumensulphate in being insoluble in absolute alcohol.

By the distillation of one part of cinnamic acid with four parts of caustic baryta, a similar fluid, cinnamen, may be obtained. Formula, $C^{16} H^{16}$; sp. gr. of the vapour = 3·55, calculated = 3·57; boils at 140°. Bromine forms with it a crystalline compound, $C^{16} H^{16} Br^4$.

Cymen is obtained pure by treating the oil of caraway with fused potassa, by which means all cuminol is separated. Cymen is colourless; boils at 165°; soluble in alcohol and æther; forms, with fuming sulphuric acid, the cymensulphuric acid: forms, with nitric acid, a new acid, which is difficultly crystallizable. Formula of cymen is $C^{20} H^{28}$; sp. gr. of vapour = $\overset{\text{I}}{4\cdot59}$, $\overset{\text{II}}{4\cdot70}$, calculated 4·69.

Cymensulphate of baryta has the formula $C^{20} H^{26}, S^2 O^5 +$

Ba O + 2 aq. It is isomeric with Delalande's camphen-sulphate, &c.—(*Annales de Chim. et de Phys.* III. t. i. p. 60.)

On account of the great length of this memoir, many imperfectly examined, and not as yet analysed substances, have been passed over in this abstract without notice. Liebig's remarks, moreover, showing that the French *type* is nothing more than *radical*, could not be extracted without encroaching too much on other important papers.

On the Action of Potassa on Indigo-blue.

Fritzsché has continued his researches on Anilin, a substance which had been previously described by Unverdorben under the name of Crystallin, as has been already mentioned. (Notices, Phil. Mag., s. 3, vol. xviii. p. 280.) The action does not commence until the potassa solution is very concentrated and has been boiled some time; the boiling point must be as high as 150°. If the solution be kept at this temperature, and pure indigo-blue added from time to time, the whole is dissolved, forming a yellowish red liquid, in which, after a time, crystals are formed; it is then better to stop the operation and allow the whole to crystallize. By this operation hardly a trace of any volatile substance is formed, and no gas evolved. A small quantity of a dark-coloured substance is formed with the yellow potash salt, from which it cannot be separated; it appears to be an accidental product. If the whole crystallized mass be treated with water, this dark coloured substance causes a formation of indigo-blue, but the excess of potassa also acts on the yellow salt, and indigo-blue is precipitated. It is better therefore to saturate the greater part of the potassa in the solution with an acid. A bluish green precipitate is formed, and by filtering a golden-yellow fluid is obtained, out of which acids precipitate a voluminous flocky reddish-brown body—chrysanilic acid. The colour of this new acid is similar to that of kermes mineral. It is not at all crystalline, assumes a darker colour by drying. It is very little soluble in water, easier soluble in alcohol; from a concentrated solution in a mixture of water and alcohol it may be obtained in a crystalline state. It dissolves in alkalies with a golden-yellow colour, but care must be taken not to use an excess of alkali, for then the fluid becomes green. The metallic salts are insoluble, lead and zinc salts are of a beautiful red colour. The formula is not as yet determined; the results obtained do not agree well with one another. By dissolving chrysanilic acid in dilute sulphuric acid two new bodies are formed, one of which crystallizes out on cooling, the other remains dissolved. The soluble one has received the name of Anthranilic acid,

It may be prepared in another way. The crystalline mass obtained by acting on indigo-blue with potassa is dissolved in alcohol, and allowed to stand exposed to the air until its greenish colour has passed into brown. A stream of carbonic acid is then passed through the solution in order to saturate the free alkali, and the alcohol is distilled off after the carbonate of potassa has been separated. On concentration, anthranilate of potassa crystallizes out in thin lamellæ, the mother liquor is separated by bibulous paper, the salt dissolved in as small a quantity of water as possible, and acetic acid added, which produces a crystalline precipitate of hydrated anthranilic acid. Anthranilic acid is, when pure, perfectly colourless, has a sweet taste even when in combination with alkali. If the decomposition of the potassa salt be conducted very slowly it may be obtained in scaly crystals, like benzoic acid. It is difficultly soluble in cold water, easier in boiling water. In alcohol and æther it is easily soluble. It melts at 135°, and sublimes; at a higher temperature it is decomposed. The alkaline salts are soluble, the copper, zinc and lead salts are crystalline powders. The formula of the hydrated acid is $C^{14} H^{12} N^2 O^3 + H^2 O$, that of the silver salt $C^{14} H^{12} N^2 O^3 + Ag O$. Hydrated anthranilic acid heated above its point of fusion is decomposed into carbonic acid and anilin, $C^{14} H^{12} N^2 O^3 + H^2 O = C^{12} H^{14} N^2 + 2 C O^2$. In an experiment it was found that the carbonic acid was 31·49 per cent. of the employed acid; according to calculation it should be 31·93. By distilling an anthranilate anilin is also obtained, but the decomposition is much more complicated; carbon is separated. It has been already stated that by the action of dilute sulphuric acid on chrysanilic acid a substance is obtained which crystallizes as the fluid cools; it is bluish black, soluble in alcohol, with a purple-red colour. After drying it loses this solubility; it is easily decomposed by alkalies; indigo-blue is formed, &c. (*Journal für Praktische Chemie*, B. 23, p. 67.)

On the Products of the Decomposition of Chloride and Bichloride of Isatin.

Erdmann has continued his experiments on these highly interesting bodies, and in the following memoir the products of decomposition are examined. When a stream of sulphuretted hydrogen is passed through a solution of chloride or bichloride of isatin, or the analogous bromides, the solution becomes colourless, and a white sediment is formed, consisting of sulphur and a new body, which may be freed from the sulphur by digestion with bisulphuret of carbon. A better method is to dissolve the chlorides in hydrosulphuret of am-

monium. By standing, or more rapidly by the application of heat, the solution deposits a white substance, which, if quickly washed with water, freed from air, may be obtained pure.

Chlorisatyd (chloride of isatyd?) is a white pulverulent substance. When dried at 120° assumes a reddish colour. Insoluble in cold water, but little in boiling water; soluble in boiling alcohol. Formula, $C^{16} H^{10} N^2 Cl^2 O^3$, *i. e.* chloride of isatin *plus* 2 atoms of hydrogen. It is not changed by the action of chlorine.

Bichlorisatyd (bichloride of isatyd) is perfectly similar to the protochloride. Formula $C^{16} H^{10} N^2 Cl^4 O^3$, *i. e.* bichloride of isatin *plus* 2 atoms of hydrogen.

Bibromisatyd is very similar, $C^{16} H^{10} N^2 Br^4 O^3$.

Chlorisatyd heated to 180° gives off water, in greater quantity at 200°; the remainder in the retort is of a brownish-violet colour. Chloride of isatin sublimes in crystals, and the remaining mass evidently contains more of the same, which, as well as the undecomposed chlorisatyd, may be extracted by boiling alcohol. The insoluble part is pulverulent, dirty violet-coloured, insoluble in water, alcohol and hydrochloric acid; soluble in caustic potash solution with yellow colour. The alkaline solution gives with hydrochloric acid a yellow precipitate, soluble in boiling water. The violet body is chloride of indin. The composition does not appear to be yet accurately determined. Bichloride of indin is obtained in the same manner, and is quite similar; bichlorisatyd, as well as bibromisatyd, are decomposed, however, at a lower temperature than the protochloride (chlorisatyd). Bibromide of indin is of a deep blackish red colour, somewhat soluble in alcohol. Chlorisatyd is easily soluble in a solution of caustic potash; out of the solution a salt crystallizes, which is chlorisatinate of potassa; out of the mother liquor acetic acid precipitates a yellow body, soluble in boiling water; by boiling the solution (which has been filtered from the yellow matter so obtained) with hydrochloric acid, flocks of a brownish violet substance are precipitated, precisely similar to the above-mentioned chloride of indin. In the fluid is chloride of isatin. Bichlorisatyd behaves exactly similar, only that the salt which first crystallizes out is not bichlorisatinate of potassa. Its solution gives with hydrochloric acid a yellowish white precipitate, soluble in hot water, from which it is deposited by cooling. Formula, $C^{16} H^{10} N^2 Cl^4 O^4$, *i. e.* isomeric with the bichlorisatinic acid. The potash salts of these acids are precisely similar; they differ in their decomposition by hydrochloric acid. The actions of nitric acid and ammonia on chloride and bichloride

of isatin are too little examined to enable us to make any abstract. Chlorine does not appear to exert any action on chloride and bichloride of isatin, when they are suspended in water, or even under the influence of sunlight. If, however, they are dissolved in alcohol, and chlorine is passed through the solution, they are soon decomposed. A thick fluid is formed. When no more of this is produced, the operation is finished. Sal-ammoniac is also formed in large quantity. In the thick oily fluid is suspended a small quantity of shining lamellæ, which may be separated by dissolving the oily fluid in alcohol, after the whole has been freed from sal-ammoniac by means of water. This crystalline substance has received the name of chloride of anil. The alcoholic solution contains hydrochloric æther; by distillation a resinous mass remains in the retort; it still contains chloride of anil, from which it may be separated by solution in alcohol; if its solution be then distilled, and heated still further, a volatile product is formed, which sublimes in needles. This is called *chloruretted chloride of indopten* (gechlortes chlorindopten). This substance, when treated with potassa, forms a new acid, *chloruretted chlorindoptenic acid* (hyperchlorindoptenic acid?). It has great similarity to the chlorindoptenic acid, but its formula is $C^{12}\,Cl^{10}$.

Chloride of anil may be sublimed if carefully heated; insoluble in water, almost so in cold alcohol; soluble in hot alcohol. It is not changed by nitric, sulphuric and hydrochloric acids. Formula $C^6\,Cl^4\,O^2$. It dissolves in solution of potassa with a purple-red colour; on cooling brownish purple-red crystals are formed, which contain chloranilic acid. The insoluble red silver salt is $C^6\,Cl^2\,O^3 + Ag\,O$. The potassa salt is $C^6\,Cl^2\,O^3, K\,O + H^2\,O$. It explodes on heating. The metallic salts are insoluble. On adding hydrochloric or sulphuric acid in excess to the warm solution of chloranilate of potassa, reddish grains, or sometimes yellowish-red lamellæ of chloranilic acid are precipitated. Dried at 125° it contains one atom of water, the crystals contain two atoms.—(Conclusion in our next.)—(*Journal für Praktische Chemie*, xxii. p. 257–299.)

On Catechin (Catechinic Acid) and Pyrocatechin (Pyrocatechinic Acid).

Wackenroder has examined the properties of catechin, Zwenger the properties and the chemical composition, and Hagen the constitution alone. The formulæ obtained agree neither with one another, nor with those formerly obtained by Svanberg, we need only therefore give those proposed.

	Svanberg.	Zwenger.	Hagen.
Catechin, dried *in vacuo*,	$C^{15} H^{12} O^{6}$		$C^{14} H^{18} O^{9}$
Dried at 100° C		$C^{20} H^{20} O^{9}$	$C^{14} H^{14} O^{7}$
Fused catechin		$C^{20} H^{18} O^{8}$	
Combined with oxide of lead			$C^{14} H^{12} O^{6}$

The pyrocatechin has been discovered and examined both by Wackenroder and Zwenger. It is obtained by heating catechin above its point of fusion; white acid vapours are evolved, and a more or less brown coloured acid fluid distils over; by evaporation the pyrocatechin may be obtained crystallized, and may then be purified by sublimation. Melts at 20° C.; easily soluble in water, alcohol and æther. The aqueous solution is easily decomposed by exposure to the air. Decomposed by alkalies and their carbonates. Formula $C^{6} H^{6} O^{2}$. In combination with oxide of lead, $C^{6} H^{4} O$.—(*Annalen der Chemie und Pharmacie*, xxxvii. pp. 306, 320, 336.)

XXX. *On some Analogies between the Phænomena of the Chemical Rays, and those of Radiant Heat.* By John William Draper, *M.D., Professor of Chemistry in the University of New York**. [Illustrated by Plate I.]

(1.) IT is the object of this memoir, to establish some striking analogies which exist between the phænomena of the chemical rays and those of radiant heat.

(2.) As most of the experimental illustrations which I shall here give depend upon the use of M. Daguerre's preparation, (though I have numerous others which serve to extend these truths to other combinations, and which will be published in due time,) I shall also incidentally give what appears to be the proper theory of the Daguerreotype.

(3.) Without saying anything of the laws of reflexion, refraction, polarization, and interference, to which these rays are subject, the study of which I commenced more than five years ago on paper rendered sensitive by the bromide of silver, further than that a general similitude holds in all these cases between the rays of heat and the chemical rays, I shall at present confine my observations to establishing the following propositions.

(4.) 1st. That the chemical action produced by the rays of light, depends upon the absorption of those rays by sensitive bodies; just as an increase of temperature is produced by the absorption of those of heat.

* Communicated by the Author.

(5.) 2nd. That as a body warmed by the rays of the sun gradually loses its heat by radiation, or conduction, or contact with other bodies, so likewise, by some unknown process, photographic effects produced on sensitive surfaces are only transient, and gradually disappear.

(6.) 3rd. That, as when rays of heat fall on a mass of cold ice, its temperature rises degree by degree, until it reaches 32° Fahr., and there stops, until a certain molecular change (liquefaction) is accomplished, and after that proceeds to rise again, so also the chemical rays impress certain changes proportional to their quantity, up to a certain point, and there a pause ensues;—a very large amount of light being now rendered latent or absorbed, without any indication thereof being given by the sensitive preparation (as the heat of fluidity is latent to the thermometer)—a molecular change then setting in,—the increments of the quantity of light are again indicated, by changes in the sensitive preparation.

(7.) 4th. That it depends on the CHEMICAL nature of the ponderable material what rays shall be absorbed.

(8.) 5th. That whilst the *specific rays* thus absorbed depend upon the chemical nature of the body, the *absolute amount* is regulated by its OPTICAL qualities, such as depend on the condition of its surfaces, and interior arrangement.

(9.) 6th. It will be proved from this, that the SENSITIVENESS of any given substance depends on its chemical nature and optical qualities conjointly, and that it is possible to exalt or diminish the sensitiveness of any given chemical compound, by changing the character of its optical relations. We shall here meet with an explanation of some of the facts noticed by Sir J. Herschel, Mr. Hunt, Mr. Talbot, and others, respecting the increase of sensitiveness of the chloride of silver and other bodies.

(10.) 7th. That, as when radiant heat falls on the surface of an opake body, the number of rays reflected is the complement of those that are absorbed, so in the case of a sensitive preparation, the number of rays reflected from the surface is the complement of those that are absorbed.

Of the Daguerreotypes.

(11.) In relation to the condition of these tablets, I shall prove the following facts:—

1st. That metallic mercury exists all over the surface of an ordinary Daguerreotype, in the shadows as well as in the lights,—in the shadows it is as metallic mercury, in the lights as silver amalgam.

(12.) 2nd. That in an iodized Daguerreotype, as taken from

the mercury-bath, there is no order of superposition of the parts, that is to say, the iodide is neither *upon* nor *beneath* the mercury, but both are as it were in the same plane.

(13.) 3rd. That when a ray of light falls upon the surface of this preparation, through all the intervening steps, and up to the point of maximum action, no iodine is evolved from the plate, but that in the common Daguerreotype the light communicates a tendency to the atoms of the iodide to yield up to the mercurial vapour their silver, whilst the iodine retires and combines with the unaffected silver around. It follows, that when such a plate is withdrawn from the mercurial vapour, there is all over it an uniform film of iodide of silver, of the very same thickness as at first, and that this has happened through a *direct corrosion* of the silver, by the iodine, whilst it was undergoing the mercurial operation.

(14.) I pass at once to the proofs of these several propositions, commencing, for the sake of perspicuity, with those relating to the Daguerreotype first: and 1st, *That metallic mercury exists all over the surface of an ordinary Daguerreotype, in the shadows as well as in the lights,—in the shadows it is as metallic mercury, in the lights as silver amalgam.*

(15.) I took a plated copper three inches by four in surface, and having prepared it with care, I exposed half of it to the diffused light of the day, screening the other half; it was then mercurialized at 175° Fahr., the iodide removed by hyposulphite of soda and washed. And now, a plate on which a gold-leaf was spread, was placed over it, but separated, as shown in Plate I. fig. 1., in the points *a*, *b*, *c* by three slips of glass. By means of a spirit-lamp the photographic plate *a*, *b*, *c* was heated, and the gilded plate *g k* kept cool, by occasionally wetting it. On parting the plates, it was perceived that faint but distinct traces of whitening were visible all over the gold, as well on that part which was over the whitened half of the photograph, as over that which was unchanged.

(16.) But as it might happen that the mercury diffused itself laterally past the imperfect obstacle *b*, I made the following decisive trials:—

I iodized three silver plates, A, B, C, each three inches by four in surface, conducting the processes for each in the same way; and having exposed each for two minutes to a faint daylight, I laid them aside in the dark, to be presently used as test plates, in lieu of the gilded plate (*g*, *k*).

Then I took three other plates, D, E, F, of the same size, and conducting the preparatory processes for each as before,

I iodized D in the dark, and mercurialized it forthwith at 170° Fahr., taking the utmost care that not a ray of light should be suffered to impinge upon it.

E was iodized, and exposed for two minutes to diffused daylight, and then mercurialized at 170° Fahr.

F was iodized, and exposed to the sun until it began to turn brown, an effect occurring almost at once. It was then mercurialized at 170° Fahr.

All these plates, then, had their sensitive coating removed by hyposulphite, and were thoroughly washed in distilled water and dried.

(17.) I had therefore three plates, representing accurately the conditions proposed to be investigated. D was in the condition of the most perfect shadows, E in that of the highest lights, and F solarized. In appearance D was black, E was white, and F bluish-gray.

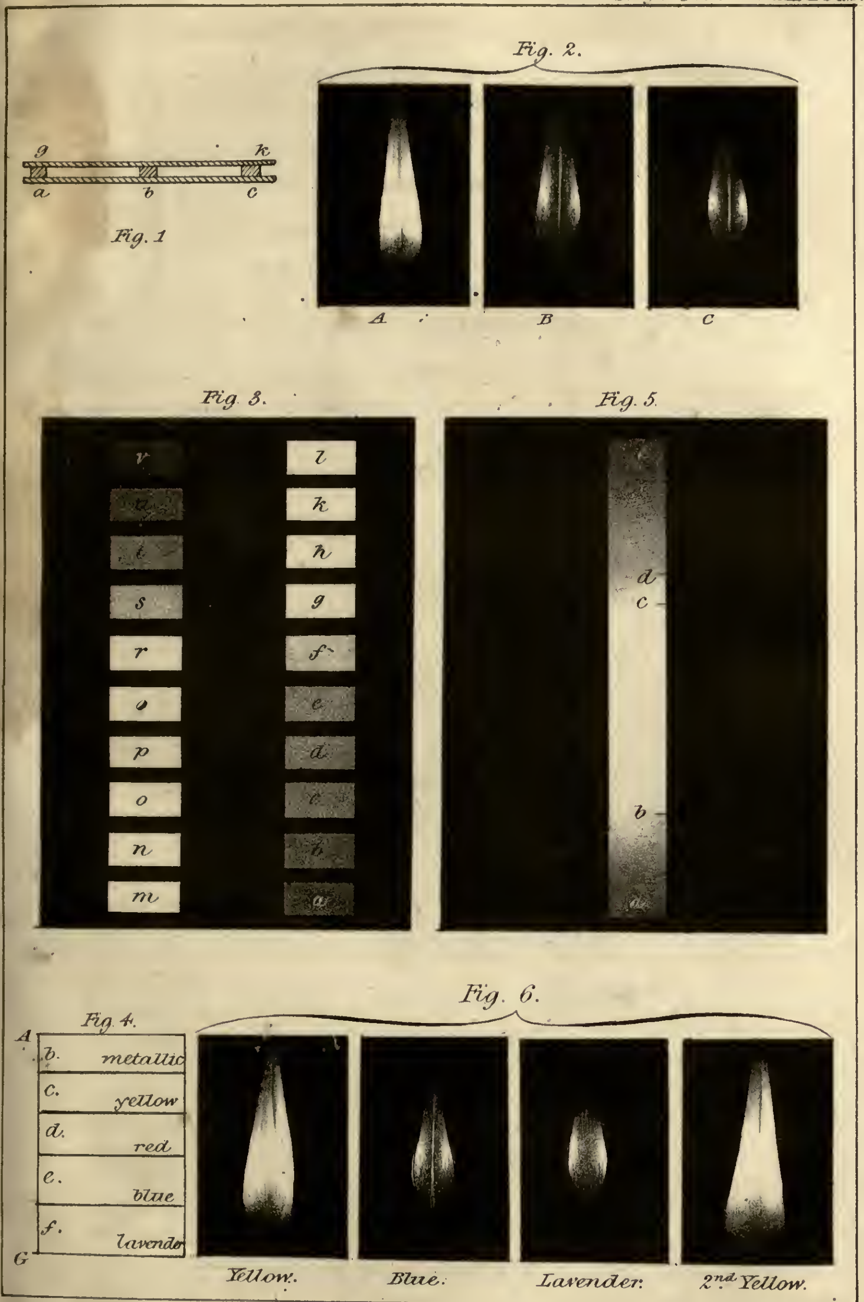
Upon D, E, F, I placed A, B, C respectively, separating each pair of plates one-sixteenth of an inch, or thereabouts, by slips of glass. Then I laid them on the level surface of the sand-bath, the test plates being kept cool by sponging occasionally with water. Temperature of the sand 200° Fahr., duration of the experiment fifteen minutes.

On examination, A, B, C were all found powerfully mercurialized, nor did there seem to be any difference between them.

(18.) I consider, therefore, that the shadows, the demitints, the lights, and the solarized portions of a Daguerreotype, are covered with mercury; for at a temperature of 200° Fahr. they all evolve it alike, a sufficiency of vapour rising from the parts that have not been exposed to the light, to bring a plate that has been so exposed to its maximum of whiteness*.

(19.) In a former Number of this Journal (s. 3. vol. xvii. p. 218), I described a remarkable effect which I had noticed in these investigations, that if an object such as a wafer be laid upon a piece of cold glass or metal, and you breathe once on it, and as soon as the moisture has disappeared remove the object, and breathe again on the glass, a spectral image of the wafer will make its appearance. The impression thus communicated to the surface, under certain conditions, remains there for a long time. During the cold weather last winter, I produced such an image on the mirror of my heliostate; it could be revived by breathing on the metal many weeks afterwards, nor did it finally disappear until the end of several months.

* I believe that the most delicate test for the presence of mercury, is a slip of silver iodized to a yellow colour, and exposed for two or three minutes to a weak daylight.



J. Basire, lith.

Professor Draper on the Analogies of Radiant Heat and the Chemical Rays.

(20.) I do not at present know what is the reason of this result, but the analogy between it and the arrangement of mercurial globules, which cover the surface of a Daguerreotype, is too striking to be overlooked. It proves to us that surfaces may assume such a condition as to affect the deposition of vapours upon them, so as to give rise to the reproduction of appearances of external forms. I gave therefore particular attention to this point, but eventually found that silver exists in an ordinary Daguerreotype, in connexion with the mercury all over the plate, in a less proportion in the shadows, and in a greater proportion in the lights. This result was, however, only obtained after the following fact was discovered—that the mucilage of gum-arabic, when slowly dried in a thin layer on the surface of a Daguerreotype, splits up in shivers, bringing along with it the white portions of the picture, and leaving the plate clean.

(21.) Having therefore prepared three plates, D, E, F, exactly as before (16.), I poured on them a solution of gum, drained them so as to leave only a small quantity, and let them dry slowly over the sand-bath. The gum separated readily, and lay in chips on the surface of each plate; it was easily removed to three sheets of paper, by tapping with the finger on the back of the plate. Each was then treated alike as follows:—

(22.) The gummy matter was incinerated on a platinum leaf, and the remaining ashes transferred to a test tube, half an inch in diameter. One drop of nitric acid and one drop of water were added; it was boiled over a small flame, and diluted with a little water. Dilute muriatic acid was now added, and the chloride of silver immediately fell. In repeating this, it is necessary to attend to the state of dilution of the acid, for if too strong it wholly dissolves the minute quantity of chloride of silver generated.

(23.) As, from the minuteness of that quantity, it was impossible to obtain a direct quantitative analysis, I adopted the foregoing method, and added the dilute acid to all three tubes at the same time. In D there was a faint opalescence, in E and F a cloud; but I could not always determine whether the deposit of E or F was most copious, sometimes the one and sometimes the other appearing to have a slight advantage.

(24.) I conclude, therefore, that whilst the whole surface of the plate is coated with mercury, it exists as silver amalgam chiefly in the lights, and as uncombined mercury chiefly in the shadows, and in a mixed proportion in the demitints;

and that when a plate is solarized, both free mercury and amalgam are present.

(25.) Such is the state of surface in a Daguerreotype, *recently formed.* In the course of time, however, a great portion of the mercury that is in the shadows, and also free in the lights, evaporates away. When the picture has thus changed, the shadows are metallic silver, and the lights silver amalgam.

(26.) 2nd. *That in an iodized Daguerreotype, as taken from the mercury-bath, there is no order of superposition of the parts, that is to say, the iodide is neither upon nor beneath the mercury, but both are as it were in the same plane.*

Soon after I had ascertained the action of gum-arabic, some of it was applied to the surface of a plate, on which an impression had just been formed in the mercury-bath. This was without removing the coat of iodine. On drying it, the gum chipped up, as was expected, bringing away with it all the lights of the picture, and leaving an uniform coat of yellow iodide of silver beneath. It seems, therefore, that the film of iodide coheres more strongly to the metal plate than the amalgam; and further, from this result we should judge that the amalgam is *on the surface* of the iodide.

(27.) But this is not true; for on three different occasions I have found that when Russian isinglass was employed instead of gum, for purposes presently to be related (34.), the isinglass, from its stronger cohesive power, chipped off in the act of drying, tearing up the yellow film from end to end of the plate, and leaving the amalgam constituting the lights undisturbed. It is here to be understood that this action takes place without the *smallest disturbance* of the lights and demitints, the plate remaining in all the beauty and brilliancy and perfection that it would have had, if it had been carefully washed in hyposulphite of soda.

(28.) This is a result, however, which I cannot produce with uniformity. Most commonly the lights are torn up with the iodide. Had it occurred but once, I should still have cited it with decision, for from the very character of it, it is impossible to be mistaken, or to commit an error of judgement. It proves to us that the film of iodide may be mechanically *torn off* from the metallic surface as perfectly as it can be *dissolved off* by chemical agents,—a singular fact.

(29.) This result, therefore, proving that we can tear off the film of iodide and leave the amalgam, can only be coordinated with that (26.) by gum water, in which the amalgam is removed and the iodide left, by supposing that

there is not anything like a direct superposition in the case, and that the particles of amalgam and iodide lie as it were side by side.

(30.) 3rd. *That when a ray of light falls upon the surface of this*, &c. &c.

There is no difficulty in proving this directly, and the indirect evidence is copious. If we lay a piece of paper imbued with starch on an iodized plate, and expose it to the sun, although the plate presently assumes a dark olive green colour, the starch remains uncoloured.

(31.) This dark substance is probably a subiodide of silver; the iodine therefore which has been disengaged from it not having been set free, must have necessarily united with the adjacent metallic silver,—this, for very obvious reasons, there is no difficulty in admitting.

(32.) Now, therefore, when a photogenic impression existing on the surface of a plate in an invisible state is brought out by the action of mercury vapour, we easily understand how this is effected. No iodine is ever evolved. But each atom of iodide of silver, that has been acted on by the light, yields to the attraction of the mercury its atom of silver, and the iodine thus set free unites with the metallic silver particles around it, reproducing the same yellow iodide by *a direct corrosion* of the plate: the proofs that we have of this are two in number.

(33.) 1st. Dry some mucilage of gum-arabic on a Daguerreotype, just brought from the mercury-bath; when it has split up, we perceive that the white amalgam of silver is removed, and an uniform coat of yellow iodide of silver, of the very same thickness as at first, as is proved by its colour, is left.

(34.) 2ndly. Dry upon the same plate a solution of Russian isinglass, and when it has split up, it will be seen that it uniformly rends off with it the yellow iodide, leaving the metallic plate with an exquisite polish; and wherever the light has touched, *there it is corroded.*

(35.) These two facts, taken together, prove that in mercurializing a plate no iodine is evolved, but that a new film of iodide of the same thickness is formed, at the expense of the metallic surface.

(36.) From these facts we readily gather, that on the presence of the metallic silver the sensitiveness of this preparation mainly depends, for to the tendency which the light has impressed on the elements of the iodide to separate, is added the strong attraction of metallic silver for nascent iodine.

(37.) This corrosion or biting in of the silver plates, by the conjoint action of the mercury and iodine, gives rise to etchings that have an inexpressible charm. Could any plan be hit upon of forcing the iodine to continue its action, the problem of producing *engraved* Daguerreotypes would be solved. By another process, which will be described hereafter, I have succeeded in producing deep etchings from Daguerreotypes.

(38.) I now commence with the proofs of the leading propositions set out with in this communication.

And 1st, *That the chemical action produced by the rays of light, depends upon the* ABSORPTION *of those rays by sensitive bodies*, &c.

Without embarrassing myself here with any considerations of the tints of thin plates, or the colours of natural objects, I shall use the term absorption as expressive of a loss of radiant matter, whether that loss arises from a direct union of the luminous molecules with ponderable matter, or is rather a disappearance of effect, caused by the interference of systems of undulations.

I iodized a plate to a golden yellow colour, and exposed it to the diffused light of day, setting it in such a position that it reflected specularly, the light falling upon it through the window, to the objective of a camera-obscura, which formed an image of it upon a second sensitive plate. The beams falling upon the sensitive plate, of course exerted their usual influence upon the iodide, which, after the lapse of a short time, began to turn brown. As soon as this effect was observed, I closed the aperture of the camera, and taking out its plate, mercurialized it, but it was found that the rays reflected from the sensitive plate, although they had been converged by a lens four inches in diameter, and formed a very bright image, *had lost the quality of changing the iodide of silver.*

(39.) We see, therefore, that a ray of light which has impinged on the surface of yellow iodide of silver, has lost the quality of causing any further change on a second similar plate on which it may fall.

(40.) In the practice of photogenic drawing, this observation is of much importance, especially when lenses having large apertures are used; the rays which converge upon the sensitive plate are reflected by it in all directions, and the camera is full of light; its sides reflect back again in all directions, on the surface of the plate, these rays, which, if they were effective, must stain the plate in the shadows. But if the plate has been iodized to the proper tint, this light is wholly without action, and hence the proof comes out neat and clean.

(41.) Upon an iodized plate I received a solar spectrum formed by a flint-glass prism, the ray being kept motionless by reflexion from a heliostat, and the plate so arranged as to receive the refracted rays perpendicularly. After five minutes it was mercurialized, and the resulting proof exhibited the place of the more refrangible colours in the most brilliant hues. The lesser refrangible colours had also left their impress of a whitish aspect, but the region of the yellow was unaltered. All the different rays, therefore, except the yellow, have the power of changing this particular preparation. Now, when a number of pieces of cloth of different colours are placed in the sun-beam, they absorb heat in proportion as their colour is deeper. A black cloth, which does not reflect any of those calorific rays, becomes presently *hot*; and in the same way Daguerre's sensitive preparation absorbs all the rays which have any chemical action on it, and reflects the yellow only, which does not affect it. In this particular lies the secret of its vast sensitiveness, compared with the common preparations of the chloride and bromide of silver.

(42.) 2nd. *That as a body warmed by the rays of the sun*, &c. After a beam of light has made its impression on the iodide, if the plate be laid aside in the dark before mercurializing, that impression decays away with more or less rapidity; first the faint lights disappear, then those that are stronger.

Having brought three plates to the same condition of iodization, and received the image of a gas-flame in the camera on each for three minutes, I mercurialized one, A, forthwith; the second, B, I kept an hour, the third, C, forty-eight hours; the relative appearance of these three images is represented in fig. 2.

(43.) Those who are in the habit of taking Daguerreotypes, know how much they suffer when the process of mercurialization is deferred. To show this effect in the extreme, I took four plates, and having prepared all alike, I exposed half of the surface of each to a bright sky for eight seconds.

No. 1. mercurialized	immediately,	came out,	black solarized.
2.	in five hours,	... ...	white.
3.	twenty-two hours,	...	same effect.
4.	one hundred and forty-four,		no effect.

(44.) This last plate, on being submitted twice more to the vapour of mercury, gave an indistinct mark. On exposing a corner of it to the sun it blackened instantly, these results showing that the peculiar condition brought on by the action

of the light gradually disappears, the compound all the time retaining its sensitiveness.

(45.) Similar results are mentioned by Daguerre in the case of the changes produced on surfaces of resinous bodies, and I have noticed them in a variety of other cases. Now to whatever cause these phænomena are due, whether to any thing analogous to radiation, conduction, &c., it is most active during the first moment after the light has exerted its agency, but it must also take effect even at the very time of exposure; and it is for these reasons that it comes to pass, that when light of a double intensity is thrown upon a metallic plate the time required to produce a given effect is less than one half.

(46.) I could conceive the intensity of a ray so adjusted, that in falling upon a given sensitive preparation, the loss from this cause, this casting off of the active agent, should exactly balance the primitive effect, and hence no observable change result. Hereafter we shall find, that one cause of the non-sensitiveness of a number of bodies is to be traced directly to the circumstance, that they yield up these rays as fast as they receive them.

(47.) It needs no other observation than a critical examination of the sharp lines of a Daguerreotype proof with a magnifying glass, to show that the influence of the chemical rays is not propagated laterally on the yellow iodide of silver. Of the manifestations which these rays may exhibit, after they have lost their radiant form and become absorbed, we know but little. If they conform to the analogous laws for heat, and if the absorbing action of bodies for this agent is inversely as their conducting power, we perceive at once *why* a photographic effect, produced on yellow iodide of silver, retains the utmost sharpness without any lateral spreading; the absorbing power is almost perfect, the conducting should therefore be zero.

(48.) 3rd. *That, as when rays of heat fall on a mass of cold ice*, &c. &c.

Although in the sun the iodide of silver blackens at once, this is only the result of a series of preliminary operations.

When we look at a Daguerreotype, we are struck with the remarkable gradation of tint, and we naturally infer that the amount of whitening induced by mercurialization, is in direct proportion to the amount of incident light; otherwise it would hardly seem that the gradation of tones could be so perfect.

(49.) But in truth it is not so. When the rays begin to act on it, the iodide commences changing, and is capable of be-

ing whitened by mercury. Step by step this process goes on, an increased whiteness resulting from the prolonged action or increased brilliancy of the light, until a certain point is gained, and now the iodide of silver apparently undergoes no further visible change; but another point being gained, it begins to assume, when mercurialized, a pale blue tint, becoming deeper and deeper, until it at last assumes the brilliant blue of a watch-spring. This incipient blueness goes under the technical name of solarization.

(50.) The successful practice of the art of Daguerreotyping, therefore, depends on limiting the action of the sun-ray to the first moments of change in the iodide; for if the exposure be continued too long, the high lights become stationary, whilst the shadows increase unduly in whiteness, and all this happens long before solarization sets in.

(51.) Let us examine this important phænomenon more minutely. Having carefully cleaned and iodized a silver plate three inches by four in size, it is to be kept in the dark an hour or two.

By a suitable set of tin-foil screens, rectangular portions of its surface, half an inch by one-eighth, are to be exposed at a constant distance to the rays of an Argand gas-burner (the one I have used is a common twelve-holed burner), the first portion being exposed fifteen seconds, the second thirty seconds, the third forty-five seconds, the fourth sixty seconds, &c. &c.

We have thus a series of discs or spaces upon the plate, *a*, *b*, *c*, *d*, fig. 3, each of which has been affected by known quantities of light; *b* being affected twice as much as *a*, having received a double quantity of light; *c* thrice as much as *a*, having received a triple quantity, &c. &c.

The plate now is exposed to the vapour of mercury at 170° Fahr. for ten minutes; the spaces or discs all come out in their proper order, and nothing remains but to remove the iodine.

(52.) An examination of one of these plates thus prepared, shows us* that, commencing with the first space *a*, we discover a gradual increase of whitening effect until we reach the seventh; that a perfect whiteness is there attained; that, passing on to the sixteenth, no increase of whitening is to be perceived, although the quantities of light that have been in-

* It is impossible to represent these changes in a drawing, which is simply black and white; it will be understood that the characteristic distinction of the spaces from the sixteenth to the twentieth, for example, depends on their assuming a *blue* tint, which continually deepens in intensity.

cident and absorbed, have been continually increasing; but as soon as the light thus latent has reached a certain quantity, visible decomposition sets in, indicated by a blueness, and the sensitive surface once more renders evident the increments of incident light.

(53.) Or, by presenting a plate covered with a screen to a sky that is clear or uniformly obscured, and with a regular motion, withdrawing the screen deliberately from one end to the other, and then suddenly screening the whole; it is plain that those parts first uncovered will have received the greatest quantity of light, and the others less and less. On mercurializing, it will be seen that a stain will be evolved on the plate, as is represented in fig. 5; from *a* to *b* the changes have been successive; from *b* to *c* no variation in the amount of whitening is perceptible; at *d* solarization is commencing, which becomes deeper and deeper to the end, *e*, of the stain.

(54.) The plate from which the drawing of fig. 5 is taken, gives from *a* to *b* ten parts, from *b* to *c* seventeen parts, from *d* to *e* twelve parts; we perceive therefore how large an amount of light is absorbed, and its effects rendered latent, between the maximum of whiteness being gained, and solarization setting in.

(55.) 4th. *That it depends on the* CHEMICAL *nature of the ponderable material what rays shall be absorbed.*

I had prepared a number of observations in proof of this, very much of the same kind as those which have some time ago been published in the Phil. Trans. by Sir J. Herschel. These refer chiefly to the variable lengths of the stains, impressed by the prismatic solar spectrum on different chemical bodies, and the points of maximum action noticed in them. For the present I content myself with referring to that excellent memoir for proofs substantiating this proposition.

(56.) 5th. That whilst the specific rays thus absorbed depend upon the chemical nature of the body, the absolute amount is regulated by its OPTICAL QUALITIES, such as depend on the condition of its surfaces, and interior arrangement.

I took a polished silver plate, and having exposed it to the vapour of iodine, found that it passed through the following changes of colour:—1st, lemon yellow: 2nd, golden yellow: 3rd, reddish yellow: 4th, blue: 5th, lavender: 6th, metallic: 7th, yellow: 8th, reddish: 9th, green, &c. &c. the differences of colour being produced by the differences of thickness in the film of iodide, and not by any difference of chemical quality.

(57.) It is a common remark, originally made by M. Daguerre, that of these different tints that marked 2 is the

most sensitive, and photogenic draughtsmen generally suppose that the others are less efficient from the circumstance of the film of iodide being too thick. Some suppose, indeed, that the first yellow alone is sensitive to light. We shall see in a few moments that this is very far from being the case.

(58.) Having brought nine different plates to the different colours just indicated, I received on each the image of an uniform gas-flame in the camera, treating all as nearly alike as the case permitted. I readily found that in No. 1 there was a well-marked action, No. 2 still stronger, but that the rays had less and less influence down to No. 6, in which they appeared to be almost without action; but in No. 7 they had recovered their original power, being as energetic as in No. 2, and from that declining again; this is shown in fig. 6.

(59.) Hence we see, that the sensitiveness of the iodide of silver is by no means constant; that it observes periodical changes which depend on the optical qualities of the film, and not on its chemical composition; and that by bringing the iodide into those circumstances that it reflects the blue rays we greatly reduce its sensitiveness, and still more so when we adjust its thickness so as to give it a gray metallic aspect. But the moment we go beyond this, and restore by an *increased* thickness *its original colour*, *we restore also its sensitiveness.* Here then, in this remarkable result, we again perceive a corroboration of our first proposition.

(60.) I may, however, observe in passing, that although I am describing these actions as if there was an actual absorption of the rays, and that films on metallic plates exhibit colours, not through any mechanism like interference, but simply because they have the power of absorbing this or that ray, there is no difficulty in translating these observations into the language of that hypothesis. When the diffracted fringes given by a hair or wire in a cone of diverging light are received on these plates, corresponding marks are obtained, a dark stripe occupying the place of a yellow fringe, and a white that of a blue. I found, more than four years ago, that this held in the case of bromide of silver paper, and have since verified in a more exact way with this French preparation. Similar phænomena of interference may be exhibited with the chloride of silver.

(61.) We have it therefore in our power to exalt or depress the sensitiveness of any compound, by changing its optical conditions. Until now, it has been supposed that the amount of change taking place in different bodies, by the action of the rays of light, depended wholly on their chemical constitution, and hence comparisons have been instituted, as

to the relative sensitiveness of the chlorides, bromides, oxides, and iodides of silver, &c. But it seems this liability to change depends also on other principles, which, being liable to variation, the sensitiveness of a given body varies with them. Thus this very iodide of silver, when in a thin yellow film, is decomposed by the feeblest rays of a taper, and even moonlight acts with energy; yet simply by altering the thickness of its film it becomes sluggish, blackening even in the sunlight tardily, and recovers its sensitiveness again on recovering its yellow hue.

(62.) We have now no difficulty in understanding, how in the preparation of ordinary sensitive paper great variations ensue, by modifying the process slightly, and how even on a sheet which is apparently washed uniformly over, large blotches appear which are either inordinately sensitive, or not sensitive at all. If, without altering the chemical composition of a film on metallic silver, or even its mode of aggregation, such striking changes result by *difference of thickness*, how much more may we expect that the great changes in molecular condition, which apparently trivial causes must bring about on sensitive paper, should elevate or depress its capability of being acted on by light! If I mistake not, it is upon these principles that an explanation is to be given of the successful modes of preparation which Mr. Talbot and Mr. Hunt have described, and the action of the mordants of Sir John Herschel.

(63.) I therefore infer,

6th. *That the* SENSITIVENESS *of any given preparation depends on its chemical nature, and its optical qualities conjointly; and that it is possible to exalt or diminish the sensitiveness of a given compound, by changing its optical relations.*

(64.) 7th. *That, as when radiant heat falls on the surface of an opake body, the number of rays reflected is the complement of those that are absorbed, so in the case of a sensitive preparation, the number of chemical rays reflected from the surface is the complement of those that are absorbed.*

This important proposition I prove in the following way:— I take a plate, A G, fig. 4, three inches by four, and by partially screening its surface whilst in the act of iodizing, with a proper piece of flat glass, I produce upon it five transverse bands, *b*, *c*, *d*, *e*, *f*; the fifth, *f*, which has been longest exposed, is of a pale lavender colour; the fourth a bright blue; the third a red; the second a golden yellow; and the first uniodized metal; the object of this arrangement being to expose at the same time and on the same plate, a series of films of different colours and of different thickness, and to

examine the action of the rays impinging on them, and the rays reflected by them.

Having prepared a second plate, B, and iodized it uniformly to a yellow, I deposit it in the camera, and now placing the first plate, A G, so that the rays coming on it through the window from the sky shall be specularly reflected to the object-glass of the camera, and the image of A G form upon B, I allow the exposure to continue until the yellow of A G is beginning to turn brown; then I shut the camera and mercurialize both plates.

In consequence of what has been said (58.), it will be readily understood, that of the bands on A G, the first one, which is the bare metal, does not whiten in the mercury vapour; the second, which is yellow, mercurializes powerfully; the third, which is red, is less affected; the fourth, which is blue, still less; and the fifth, which is lavender, hardly perceptible.

But the changes on B, which have been brought about by the rays reflected from A G, are precisely the converse; the band, which is the image of *b*, is mercurialized powerfully; that of *c* is untouched and absolutely black, *d* faintly stained, *e* whitened, and *f* mercurialized, but little less than *b*.

(65.) It follows from this, that a white stripe on B corresponds to a black one on A G, and the converse: and for the depth of tint of the intermediate stripes those of the one are perfectly complementary to the corresponding ones of the other.

By the aid of these results, we are now able to give an account of the variability of sensitiveness in photogenic preparations; the yellow iodide of silver is excessively sensitive, because it absorbs all the chemical rays that can disturb it, whilst the lavender is insensitive, because it reflects them. Under this point of view, sensitiveness therefore is directly as absorption and inversely as reflexion.

The superiority of Daguerre's preparation over common sensitive paper may now be readily understood. It absorbs all the rays that can affect it, but the chloride of silver, spread upon paper, reflects many of the active rays. The former, when placed in the camera, gives rise to no reflexions that can be injurious; the latter fills it with active light, and stains the proof all over. Hence the Daguerreotype has a sharpness and mathematical accuracy about its lines, and a depth in its shadows, which is unapproachable by the other. Moreover, the translucency of the white chloride of silver, as well as its high reflecting power, permits of particles lying out of

the lines of light being affected, the luminous material becoming diffused in the paper.

The fact, therefore, that a given compound remains unchanged even in the direct rays of the sun, is no proof that light cannot decompose it; it may reflect or transmit the active rays as fast as it receives them. It results from this, that *optical forces* can control and even check the play of *chemical affinities*. Whilst thus it appears that there are points of analogy between this chemical agent and radiant heat, we must not too hastily infer that the laws which regulate the one obtain exclusively also with the other. As is well known, there are striking analogies between radiant heat and light, but there are also points of difference, the convertibility of heat of one degree of refrangibility to another does not occur with light; there are also dissimilitudes in the phænomena of radiation and its consequences. I do not doubt, that what has been communicated in this memoir, will, by the researches of others, be greatly extended; but it is not to be expected that a complete parallel can be run between radiant heat and the chemical rays, any more than between radiant heat and light.

From the phænomena of the interference of these rays, of the sensitiveness or non-sensitiveness of the *same* chemical compound being determined merely by the fact of its thickness or thinness, these, and many other similar results, *obviously* depending upon mechanical principles, it seems to me that very powerful evidence may be drawn against the materiality of light, and its entering into chemical union with ponderable atoms. Those philosophers who have endeavoured to prove the undulatory theory, will probably find in studying these subjects cogent evidence in favour of their doctrines.

XXXI. *On the Atomic Weight of Carbon. By Professors* Redtenbacher *of Prague, and* Liebig *of Giessen**.

In the analysis by combustion of organic substances which contain carbon and hydrogen, the observation has frequently been made of late years, that the weight of the elements separately found by experiment, actually exceeds the

* Translated from the original German by Dr. J. H. Gilbert; and communicated by the Chemical Society, having been read before the Society, May 8th, 1841.

An abstract of Dr. Marchand's paper on the same subject will be found in the Proceedings of the Society, to be given in our next Number.

original weight of the matter submitted to combustion. In the analyses we possess of naphthalin by Mitscherlich, by Dumas, and by Woskresensky, this is particularly remarkable. One hundred parts of naphthalin gave to Mitscherlich,—

	1.	2.
Carbon	94·34	94·440
Hydrogen	6·26	6·225
	100·60	100·665

One hundred parts of naphthalin gave to Dumas,—

	1.	2.	3.	4.	5.
Carbon ...	94·2	94·22	94·27	94·9	94·9
Hydrogen	6·3	6·30	6·26	6·2	6·1
	100·5	100·52	100·53	101·1	101·0

And to Woskresensky, 100 parts of the same substance yielded—

	1.	2.	3.	4.	5.	6.
Carbon	94·625	94·598	95·0268	93·668	94·395	94·494
Hydrogen	6·528	6·289	5·3830	6·142	6·206	6·526
	101·153	100·897	100·4098	99·810	101·601	101·020

This constant occurrence in so many carefully conducted experiments, indicates a common source of error upon which it is dependent; it can only be attributed to two causes. One of these may be sought in the defects of the method of analysis, the other in the supposition that the products of the combustion (water and carbonic acid) have different compositions from those usually assigned to them. If indeed either water or carbonic acid contains somewhat less of hydrogen or of carbon than we at present suppose, then as we calculate from the quantities found of the former bodies, the excess in the analyses is diminished in the same proportion.

Let us suppose, for example, that carbonic acid contains only 76 carbon instead of 76·437 to 200 of oxygen, and water only 12 hydrogen instead of 12·4795 to 100 oxygen, and then we shall have no excess in any of the analyses quoted, whilst the experimental come to agree perfectly with the calculated results. Are we then entitled to make such changes in the atomic weights, proceeding as we do upon the assumed accuracy of experiments, which, from the complex nature of the apparatus, can make no claim to absolute precision; or

ought we not first to compare these with other experiments, in which this source of error is entirely avoided?

A question also arises, whether *naphthalin*, a substance the atomic weight of which cannot be determined with certainty, as it enters undecomposed into no combination, is a proper substance to select as the means of determining the atomic weight of carbon or of hydrogen? That body must indeed be rejected on this account, for it is not in our power to control our analytical results from a knowledge of the weight of its atom, that is, the sum of the atomic weights of the elements composing it.

When we also consider, that the naphthalin, which in the above experiments was submitted to combustion in a glass tube with oxide of copper, is a volatile body, that it cannot be introduced into the combustion tube, with oxide of copper that is absolutely free from moisture; and bear in mind also, that, owing to the volatility of the substance, this moisture cannot previously to combustion be removed by means of exhaustion, we cannot doubt the existence of a source of error, which must increase the per-centage of hydrogen beyond that which actually existed in the substance; for, however small the quantity of this hygroscopic moisture may be, it is nevertheless always present; it is weighed with the chloride of calcium tube, and its hydrogen added to that contained in the substance.

In all analyses hitherto conducted, even those in which the whole of the hygroscopic water had been removed as nearly as possible before combustion, by means of exhaustion, it is observed that the experiment invariably gives rather more hydrogen than is indicated by calculation. This excess amounts in good analyses to from 0·1 to 0·2 per cent. It is found, however, that this, in reference to the quantity of substance employed in analysis, is not sufficient to affect the proportion of the elements, to the extent observed in the analyses of naphthalin; the excess in those analyses is however diminished, when allowance is made in the calculation for this error.

There exists therefore some other cause affecting the determination of the equal quantity of the elementary constituents of an organic substance, in such a manner, that one of them, namely, the *carbon*, when calculated from the quantity of carbonic acid obtained by combustion, amounts to more than the weight of the carbon which is contained in the matter analysed. On this account a new determination of the *atomic weight of carbon* appears to be indispensable, and we have united, in

order conjointly to submit the atomic weight of carbon, as at present received, to a severe and accurate scrutiny.

It is known that two of the most distinguished natural philosophers, Biot and Arago, have, by means of the direct weighing of carbonic acid, fixed upon the number 1·519 for the specific gravity of that gas. Their experiments were repeated by Dulong and Berzelius, with whom, as regards skill and talent, conscientiousness and accuracy, no others can be compared. The two last observers found the number 1·524 for the specific gravity of carbonic acid; that obtained by De Saussure is 1·5269.

The atomic weight of carbon, as calculated from the first of these, is 75·530, and from the other, 76·437. There is no known gas more easily obtained in a pure state, or which can more easily be distinguished from a foreign body, than carbonic acid. Any admixture of atmospheric air, or of other gases, can only lower its specific gravity.

Experiments have lately been conducted by Rudberg on the dilatation of gases under the influence of heat*, from which he calculates that the coefficient of dilatation is somewhat less than was previously supposed; should these experiments be correct, the proof of which still remains to be made known, they do not influence the specific gravities of two gases as determined at the same temperature, even supposing the reduction to the normal temperature be made according to the coefficient of dilatation as hitherto received; if weighed at unequal temperatures, however, a difference is observed.

In the experiments of Dulong and Berzelius, atmospheric air was weighed at $20\frac{1}{2}°$ C., and carbonic acid at 18° C.; consequently the reduction of the gas to 0° C., according to the former coefficient of dilatation, gives the weight of air somewhat too high, and since this, in an equal volume, represents the *divisor*, the specific gravity of carbonic acid is estimated rather too low; in all cases, however, the differences fall within the limits of the errors of observation.

When we remember that the determinations of the specific gravities of these gases were conducted with the same balloon, the same scales and weights, and at temperatures varying very little from each other, we ought not to call in question their correctness without the strongest and most convincing reasons.

During the last twelve years, a great number of weighings of the vapours of volatile bodies very rich in carbon, have been undertaken in reference to this point by Gay-Lussac,

* See Taylor's Scientific Memoirs, vol. ii. pp. 507, 514, 543.

and also by Dumas; as, for instance, those of alcohol, æther, and acetone, the results of which agree either perfectly with the specific gravity of carbon vapour, as deduced from that of carbonic acid and of oxygen, or indicate it to be somewhat higher; thus the specific gravity of æther vapour is 2·586 by experiment, and 2·580 by calculation; and that of the vapour of alcohol is 1·6133 by experiment, and 1·600 by calculation.

In most of the observations of Dumas, the observed specific gravity of bodies very rich in carbon, is far higher than that obtained by calculation. Thus, according to the formula C_5 H_2, the specific gravity of the vapour of naphthalin is 4·4882; the experiment of Dumas, however, gives 4·528, and that of Woskresensky 4·672, from which it may with great probability be concluded, that the specific gravity of carbon vapour is rather higher than 0·42139, or 0·84279.

The above-mentioned atomic weights of carbon, which have resulted from direct experiments, are contradicted by one, which, however, we may say is quite fictitious; its adoption is based on the hypothesis that the atomic weights of simple bodies are multiples of that of hydrogen by whole numbers. The atomic weight of hydrogen is in the abstract very small, and it would be strange indeed, if this, when multiplied by whole numbers, did not in many cases give a number, within certain limits, approaching (suppose we say of one-fourth or one-eighth of its own atom) those of bodies having higher atomic weights, so that a multiple of the atomic weight of hydrogen could, without introducing an important error, be substituted for that found for other bodies. For instance, by dividing 1351·61, the atomic weight of silver, by 6·2394, the atomic weight of hydrogen, we obtain the number $216\frac{38}{62}$, that is to say, the atomic weight of hydrogen is contained about 216·5 times in that of silver; but even if one-half the atomic weight of the former is added to, or subtracted from, that of a compound of silver, so small a variation is made in the per-centage of that metal, that in many cases 216 may be taken instead of 216·5, the difference only affecting the fourth figure. The error is so small, because the atomic weight of hydrogen is itself very small. An entire misconception of the nature of the investigation respecting chemical equivalents, has led some chemists to permit themselves the license of adding to, or retrenching from, their results so much as the amount of one-half or one-fourth of an equivalent of hydrogen, seeing that it altered very little the relation of the numbers to each other, provided it led to whole numbers, which

were multiples of the equivalent of hydrogen. It is in this manner that the number 75 is arrived at as the atomic weight of carbon, that number being very nearly the multiple of the atomic weight of hydrogen, by the whole number 12.

This is doubtless an unusual mode of controlling the accuracy of an experiment, yet the numbers thus modified have been admitted by many chemists.

The fact that the atomic weights of simple bodies are very nearly the multiples of that of hydrogen by whole numbers, is in itself nothing remarkable; there are other numerical relations of this kind, which appear far more extraordinary, on the strength of which, however, no one would think of making a change in atomic weights.

Thus, if we add to the atomic weight of potassium	489·92
the atomic weight of lithium, which is	80·33
we obtain the number	570·25

and this divided by 2, gives 285·12; now this last is as near the atomic weight of sodium, which in chemical properties is a link between the two former bodies, as is the atomic weight of that body obtained by multiplying that of hydrogen by a whole number.

Again, the sum of the atomic weights of barium and of calcium, divided by 2, gives very nearly that of strontium; the sum of those of chlorine and of iodine, divided by 2, gives nearly that of bromine; and those of iron and cobalt, divided by 2, give that of manganese.

There are evidently hidden connexions in these numerical relations, with which we are not acquainted, and to take them as criteria, before they are understood, is obviously inconsistent with the true spirit of philosophy; the same must be admitted with respect to the hypothetical atomic weight of carbon, 75, for the correctness of which as yet no experience speaks. The early determination of the atomic weight of lead by Berzelius, entirely confirmed as it is by his later experiments in 1830, is in complete contradiction with the correctness of atomic weights as multiples of that of hydrogen.

His memoir on that subject is indeed highly important in reference to our own investigation, and should not be forgotten by chemists. The following is an extract *verbatim* from his memoir*.

"*Further experiments on the Atomic Weight of Lead and its Oxides.*—The reduction of a metallic oxide by means of hydrogen gas, appears so simple an experiment for determi-

* Poggendorff's *Annalen*, B. xix. S. 310–315.

ning the atomic weight of a metal, that it might be supposed the results obtained would enable us to settle the question, whether or not the atomic weight of a metal is a multiple of that of hydrogen; but the nearer we approach to absolute accuracy, the greater are the obstacles we have to overcome in arriving at such a point. Besides, the circumstance that very few bodies submitted to analysis are absolutely free from all impurity, or from the substances from the compounds of which they are separated, introduces a difficulty which is often not less than that of accurately conducting the analysis itself.

"I was of opinion that crystallized nitrate of lead, which is ignited in a platinum crucible until the nitric acid is entirely decomposed, would give perfectly pure oxide of lead; but when this oxide is reduced by means of hydrogen gas, and the metallic lead dissolved in nitric acid, lead-coloured scales, which prove to be metallic platinum, remain behind. It is true the quantity of this residue is very small, but if the result be depended upon up to the last figure, then even the smallest impurity should be avoided. Crucibles of gold and of silver were also employed, but these are oxidized, and combine with the oxide of lead, even when the salt is introduced in small quantities into the crucible previously heated to redness; indeed oxide of lead thus obtained, is impregnated with the foreign metal to a greater extent than when the calcination takes place in a crucible of platinum.

"This induced me to employ carbonate of lead prepared by precipitation, partly from the acetate and partly from the nitrate of lead, by means of the carbonate of soda; and in order to avoid all admixture of the carbonate of soda with the precipitate, an excess of the salt of lead was employed; but notwithstanding this precaution, and the perfect washing of the precipitate, the atomic weight obtained by means of reduction, oscillated in both cases between 1303·5 and 1306·0; and when the metallic lead was heated with pure water, the latter was found to contain carbonate of soda. Carbonate of ammonia, employed as the precipitant, is not objectionable on this ground; but, partly because, if special care be not taken, it is difficult to obtain that reagent perfectly free from all traces of the hydrochlorate and of the sulphate of ammonia; and also because, during the calcination, the hydrogen of the ammonia may reduce a small quantity of the oxide to the condition of suboxide, which, though not perceptible, nevertheless notably alters the result.

"By the following method I think I succeeded in obtaining perfectly pure oxide of lead. Nitrate of lead is heated to redness in a platinum crucible, until nothing but oxide of lead

remains; this is rubbed to powder, and digested in water with twice its weight of neutral nitrate of lead for some hours; the liquid is then poured off. The oxide is by this means converted into the basic nitrate ($2\,Pb\,O + N\,O_5$), which is dissolved in boiling water, the solution filtered whilst boiling hot, and then allowed to cool, when fine scaly crystals are deposited. These were collected, washed, pressed into a consistent mass, and dried. The mother liquor of these crystals contains a still more basic salt, which precipitates on mixing it with a solution of the neutral nitrate. The liquid decanted at the commencement of the process, was therefore treated in this manner in these experiments. The last precipitate, which is pulverulent, was thrown upon a filter, and allowed to drain, and, whilst yet moist, the inside of a platinum crucible was coated with it to the extent of half a line in thickness. This coating, when dry, adheres strongly to the crucible. Since this basic salt does not fuse at the temperature required completely to decompose it, there is formed in this way a quantity of oxide of lead, which it is true contains platinum when in contact with the vessel, but the inner portions are quite free from that metal. In the crucible thus lined, the basic salt is laid in single pieces, so that after the calcination is completed, the oxide can be removed without any admixture of that lining the vessel; for the crucible containing the salt is, previous to the calcination, put into a larger one, having a cover, and the whole is then placed in charcoal, and heated to redness, at which temperature the oxide does not fuse. It is very easily known when the salt is entirely decomposed, for it is first converted into minium, which appears almost black when red-hot; the smallest particle of it can therefore be plainly distinguished. After this change is completed, the heat is continued for a full half-hour, and then the crucible is removed. The oxide thus obtained is of a beautiful lemon-yellow colour, and it does not in the least adhere to the coating. It still possesses the glistening appearance of the decomposed scaly crystals. It dissolves in dilute acetic acid without changing the colour of that liquid in the smallest degree, or leaving any residue, which proves that it contains no minium. The solution, moreover, is not rendered turbid by the addition of nitrate of silver.

"When this oxide was dissolved in nitric acid, precipitated by sulphuric acid, the filtered acidulous liquid concentrated, and the excess of sulphuric acid expelled, sulphate of lead remained behind, from which water did not extract any traces of a copper salt, and it was neither coloured nor rendered

turbid by caustic ammonia. The lead obtained by reducing this oxide by means of hydrogen, dissolved in nitric acid, without leaving any residue. The oxide is therefore pure.

"The oxide in masses, not in powder, is introduced into a glass bulb blown upon a barometer tube, and in this it is weighed. In order to expel all moisture, the bulb was heated over a spirit-lamp, until the oxide assumed a dark orange red colour, and a stream of dry air was then passed through it, after which it was allowed to cool. The oxide regains its lemon-yellow colour by this treatment, proving that no minium is found, for which indeed the temperature is not sufficiently high. The oxide prepared in this manner is very little hygroscopic, so that 13 to 14 grammes contain at the utmost from 1·5 to 2 millegr. of moisture.

"The hydrogen gas was evolved from distilled zinc and sulphuric acid, and was, previous to entering the bulb, conducted through a solution of oxide of lead in caustic potass, and also through a tube containing coarsely powdered hydrate of potass. At the commencement of the operation, and until about two-thirds of the oxide were reduced, the temperature was not raised so high as to make the bottom of the bulb red-hot; when this precaution is neglected beyond certain limits, a portion of the oxide of lead combines with the glass, and is not subsequently reduced. It was on this account that the oxide was introduced in masses, which only touched the glass at a few points; free access between the pieces was moreover by this method afforded to the hydrogen. The first effect of the hydrogen gas is to convert the oxide into suboxide, owing to which the masses become of a dark gray colour; their form and size are not otherwise changed, although the temperature is higher than is required for their fusion, supposing them to consist of metallic lead. As soon as the glass begins to be red-hot at the bottom, small globules of lead are seen to form, and the whole is gradually converted into fused metallic lead. Of the portions of oxide of lead which were afterwards analysed, only two were obtained by one and the same operation; all the others were separately prepared, so that a fault in the preparation of the oxide cannot introduce a constant error into all the analyses; this, however, might easily occur if the same specimen of oxide had been employed in all the different analyses.

Nos.	Ox. of Lead in grs.	Lead.	Oxygen.	Atomic Weight of Lead.	Lead. In 100 parts.	Oxygen. In 100 parts.
1.	6·6155	6·1410	0·4745	1294·202	92·8275	7·1725
2.	8·0450	7·4675	0·5775	1293·074	92·8222	7·1778
3.	13·1465	12·2045	0·9420	1295·695	92·8346	7·1654
4.	14·1830	13·1650	1·0180	1293·222	92·8224	7·1776
5.	14·4870	13·4480	1·0390	1294·315	92·8201	7·1779
6.	14·6260	13·5775	1·0485	1294·946	92·8314	7·1686
			Mean	1294·259	92·8277	7·1723

"These results, which range about between 1293 and 1296, appear to prove that the atomic weight of lead lies between those two numbers. The mean of these experiments differs so little from the number arrived at in my former ones, namely, 1294·489, that I consider it unnecessary to alter the latter.

"If the atomic weight of hydrogen is 12·5, then the atomic weight of lead, supposing it to be a multiple of that number, is exactly either 1287·5, or 1300; and if either of these numbers be the true one, it appears to me that my results must have oscillated about one of them; instead of which, they, as we see, oscillate about a number which lies precisely midway between the above-named."

It may therefore be concluded, that the fact of an atomic number being a multiple of the equivalent of hydrogen is no proof of its exactness.

There are other means of controlling and estimating anew the atomic weight of carbon. The direct method, viz. that of burning a known quantity of pure carbon, and ascertaining the quantity of carbonic acid formed, is but little fitted for the solution of the point in question, since a complex apparatus is required for collecting the gas, a circumstance which lessens the dependence to be placed in such determinations. If, indeed, it be remembered, that even in operating on several grammes of carbon, the variations generally amount to from 8 to 10 millegrammes, it will be evident that a complex apparatus offers no security for absolute accuracy. We have therefore selected a different method; an ordinary one, it is true, but which has been hitherto generally acknowledged to be the most certain and the most free from error in analytical chemistry.

We can, for instance, estimate with great certainty the atomic weights of many organic compounds, namely, numerous organic acids, by determining the proportions in which

they combine with oxide of silver, or, what is the same thing, with metallic silver. These organic acids contain several atoms of carbon, combined with oxygen and hydrogen in certain proportions, which can be ascertained with great facility. It is evident, that if the formulæ of these acids are known with certainty, we obtain the sum of the weights of the atoms of carbon, by subtracting the sum of the atoms of oxygen and hydrogen which they contain from the atomic weights of their compounds, as accurately determined by means of their silver salts. The sum of the atoms of carbon thus ascertained, must, if the formerly received number for carbon be correct, be a multiple of it by a whole number, or it must indicate how far that number differs from the true one. There is no substance, the atomic weight of which we believe to be known with greater certainty, and more precisely determined, than that of silver; the continued and important applications of it by Gay-Lussac, in his assay of silver in the wet way; indeed we may say, every one of his experiments on that subject is a fresh proof of its correctness.

With respect to the atomic weight of hydrogen, there is strong reason for thinking it to be rather less than was formerly supposed. In the three last analyses of water by Berzelius and Dulong, the following numbers were obtained:—

	1.	2.	3.
Oxygen	88·942	88·809	88·954
Hydrogen ...	11·058	11·191	11·046
	100·000	100·000	100·000

Supposing the atomic weight of hydrogen to be 6·2398, then the limits of error lie between the numbers 6·3055 and 6·2085. The difference between these numbers is 0·0970, which would either increase the atomic weight of carbon from 76·437 to 76·534, or reduce it to 76·340. If we take the mean, which is 6·2398, for hydrogen, then the limits of error in the determination of the atom of carbon fall in the second decimal place. Such variations result, as we know, from errors of observation.

The method which we have selected, enables us to estimate the atomic weight of carbon in the condition in which it is contained in organic compounds; and it requires only three weighings:—first, that of the vessel in which the salt is burned; secondly, that of the silver salt; and, thirdly, that of the residual silver. In these three weighings no apparatus is changed, and they take place in one and the same porcelain vessel, of which the weight is constant. The height of the

barometer and the proportion of humidity in the atmosphere, exert no influence on this experiment, and the silver which remains behind is not hygroscopic.

The only precaution in these experiments, to which the greatest attention must be directed, is, of course, the rigid purity of the salt, and consequently the entire absence of all hygroscopic water.

It is at all times a difficult task to obtain a chemical compound in a state of absolute purity; to do this in our experiments, was, however, very important, as even the most minute admixture of a foreign substance must increase the found weight of the atom of carbon.

We were soon convinced that there are but few silver salts available in determinations of this kind; most of them are obtained in the form of caseous, or pulverulent precipitates, which enter into combination with a part of the precipitant. The quantity of foreign matter which adheres to these precipitates is so small, that it does not generally render the estimations of atomic weights incorrect, but, as before observed, we are here obliged to avoid every source of fallacy. We selected from those silver salts which are perfectly crystalline, which retain no water when dried at the ordinary temperature, which are not hygroscopic, and which (and this is a character of great importance) do not sputter when heated. The salts employed should moreover leave behind no carburet of silver after calcination.

There are, as stated above, but few silver salts which are not liable to one or other of these sources of error.

The cyanide of silver, for instance, is easily obtained in a pure state, by precipitating the nitrate of that metal by means of hydrocyanic acid; it can even be obtained in large shining tables, by allowing to cool slowly a hot mixture of hydrocyanic acid, with a dilute ammoniacal solution of silver; carefully washing the crystals, thus obtained, with solution of ammonia, and drying them at 120° C. By the latter treatment, and even at the ordinary temperature, all the ammonia is got rid of, and the crystals become opake milk-white, without suffering any other change; they are nevertheless in practice not fitted to be employed in our experiment, for they do not appear to be entirely decomposed by heat.

When the cyanide of silver is first heated, it melts without evolving any gas; by increasing the temperature cyanogen is given off, and a basic cyanide is formed; at a certain point beyond this, flame is developed, nitrogen is then evolved, the flame being at the same time as it were extinguished; and fused carburet of silver, of a dull white colour, remains be-

hind, from which the carbon cannot be expelled by subsequent calcination. It is true, the carbon burns at the surface of the carburet, leaving a stratum of pure silver, but this protects the inner part from the contact of oxygen; and when this residue is dissolved in dilute nitric acid, a net-work of pure carbon remains.

The behaviour of the benzoate of silver is very similar to that of the cyanide; it is easily obtained perfectly pure, in beautiful shining crystals, which are not hygroscopic; when this salt, to the amount of 7 to 12 grammes, is heated, it melts and is decomposed; but even after being kept at a red heat for twelve hours, a very considerable quantity of carbon is found in the residue of silver.

The oxalate of silver appeared at first to be the most applicable of any in these determinations, but it is almost impossible to obtain it anhydrous, besides which, when heated in large masses, it deflagrates in the same manner as the fulminating mercury. We could, it is true, ascertain its composition by decomposing it, by means of hydrochloric acid, but we consider it important to bring no foreign element into these determinations. In the following experiments, the *acetate*, the *tartrate*, the *racemate*, and the *malate* of silver were employed.

Acetic acid, owing to its volatility, is easily obtained quite pure. We prepared it by decomposing some brilliantly white sugar of lead, which had been frequently recrystallized, in the usual manner, by means of sulphuric acid. The acetic acid thus obtained, which was free from sulphurous acid, was once more rectified over binoxide of manganese in excess. The pure acid was then partly saturated with ammonia, and precipitated by nitrate of silver in the warm. A shining white precipitate was obtained, possessing the form of small laminæ, having a silvery lustre; this was perfectly washed on a filter, dissolved in hot water, the solution filtered, and left to cool in a glass vessel, when at the bottom, at the surface, and on the sides, broad shining needles of acetate of silver, of an inch in length, were formed; these were again washed with pure water, dried in the air, finely powdered in an agate mortar, and exposed to a stream of dry air at 103° C., until the weight remained unaltered.

In each individual analysis, the weighed salt was once more heated in a water-bath for an hour, and then allowed to cool under a bell-glass with concentrated sulphuric acid; after which it was again weighed. It never occurred in our experiments, that the weight was perceptibly altered by this treatment; this precaution was nevertheless uniformly adopted.

The weighings as well as the burnings were conducted in a thin crucible of Meissner porcelain, which was covered by a platinum lid. All our experiments were fortunately performed with the same crucible, the weight of which therefore was controlled whenever weighings were made.

The calcination of the acetate of silver proceeds very easily, without either swelling up or spirting. The salt at first becomes gray, and, on cautiously heating acetic acid, distils off, and then the salt assumes a brown colour; at length, when the odour of the acid is no longer observable, there remains a gray skeleton of silver, which retains the form of the salt burnt. If the heat be now increased, and the lid raised, so as to admit a current of air, a visible glowing is observed throughout the whole mass, and there remains a spongy mass of shining white metallic silver.

After cooling, the crucible with the silver is weighed, heated to redness anew, weighed again, and so on, until not the slightest change of weight is exhibited. The absence of carburet of silver was always particularly demonstrated by dissolving the silver in dilute nitric acid.

The weighings were effected with a balance, which indicated quite distinctly half a millegramme, even when loaded with 20 grammes. The weights (made by Oerthing of Berlin) were carefully compared previously to employment, and were found to exhibit no appreciable variation in their subdivisions. Lastly, we adopted the precaution of conducting one-half of the experiments with newly prepared salt, by which means the fact, that the numbers yielded by salts prepared by different operations agree, is rendered very obvious.

The tartrate of silver is not easily obtained in a crystalline form. When the nitrate of silver is precipitated in the cold by a solution of pure Rochelle salt, from which, by the addition of a little nitric acid, the weak alkaline reaction which it generally possesses has been removed, a caseous and not a crystalline precipitate is formed. On the other hand, if the precipitation is effected by mixing boiling hot and dilute solutions, the liquid becomes brownish, but not turbid, and on cooling, metallic silver in brown laminæ falls down. Again, when a dilute solution of nitrate of silver is heated to from 80° to 85° C., and to this a hot concentrated solution of the tartrate of potass and soda is added, a precipitate falls, which, by agitating, at first disappears; if the addition of the tartrate of potass and soda be discontinued as soon as the precipitate is permanent, and is not redissolved, fine scales of the tartrate of silver separate on cooling, which, after being well washed and dried, are very white, and have a metallic lustre, resem-

bling that of polished silver. In this process the liquid must always contain a slight excess of the nitrate of silver.

The pure tartrate of silver was dried, and its silver determined by means of calcination, observing the same rules of precaution as in the case of the acetate. By gently heating the salt pyrotartaric and carbonic acids distil off, and there remains, without spirting or swelling, a spongy mass of bright metallic silver, which, when washed with water, yield to it no trace of alkali. Of four determinations which we made, two were with salts prepared by different operations; that employed in the fifth, was prepared with very special care by a gentleman in the laboratory at Giessen, in the course of his investigation on the constitution of organic acids, and with a view to discover, if possible, some difference between the atomic weights of the tartaric and racemic acids. For the preparation of the racemate of silver, very pure racemic acid was half neutralized by ammonia, the resulting sparingly soluble acidulous salt was washed with water, redissolved in water containing ammonia, and again thrown down by means of nitric acid. The acid racemate of ammonia thus obtained was brilliantly white, and perfectly pure. This was employed in the preparation of crystallized racemate of silver, exactly in the manner described for the tartrate of that metal. These two salts do not differ from one another in appearance, but the racemate is less soluble in hot water than the tartrate.

We prepared the malate of silver by means of the acid malate of lime, and nitrate of silver. The acid malate of lime is, by virtue of its very unequal solubility in hot and cold water, easily obtained perfectly pure. Nitrate of silver is mixed with a warm solution of this acid malate, when immediately a granular crystalline, very heavy precipitate falls. Since, after frequently washing this silver salt with water, the fluid still contained traces of lime, the whole precipitate was dissolved in dilute nitric acid, and to this solution ammonia was added drop by drop, taking care that free acid should always be present in excess. The malate of silver is in this case free from lime and ammonia, and is, after continued washing, quite pure.

The malate of silver is decomposed when heated; it fuses, at the same time evolving fumaric acid, carbonic acid, and water, and there remains behind a porous cake of silver, which is free from carbon.

The following are the results of our experiments, represented in a tabular form.

Nos.	Weight of the Salt in Grammes.	Weight of Silver in Grammes.	In 100 Parts. Silver.	Oxide of Silver.	Acid.	Atomic Weight of the Salt.	Sum of Four Atoms of Carbon.	Atom of Carbon.
				ACETATE OF SILVER.				
1.	4·8735	3·1490	64·615	69·396	30·604	2091·790	302·745	75·686
2.	7·5870	4·9030	64·624	69·402	30·598	2091·504	302·458	75·615
3.	6·4520	4·6950	64·623	69·405	30·595	2091·511	302·465	75·616
4.	5·7905	4·7415	64·614	69·395	30·605	2091·804	302·758	75·689
5.	4·1000	2·6490	64·610	69·390	30·610	2091·951	302·905	75·726
	28·803	18·612	64·618	69·399	30·601	2091·680	302·634	75·658
				TARTRATE OF SILVER.				
1.	3·8400	2·2770	59·297	63·684	36·316	2279·390	302·824	75·706
2.	2·7597	1·6365	59·299	63·688	36·312	2279·270	302·704	75·676
3.	3·2356	1·9183	59·287	63·674	36·326	2279·751	303·185	75·799
4.	5·4217	3·2147	59·293	63·682	36·318	2279·530	302·964	75·741
5.	0·9630	0·5710	59·293	63·681	36·319	2279·505	302·939	75·735
	16·220	9·6175	59·294	63·682	36·318	2279·491	302·925	75·731
				RACEMATE OF SILVER.				
1.	5·2640	3·1210	59·290	63·676	36·324	2279·670	303·104	75·776
2.	9·2668	5·4945	59·292	63·679	36·321	2279·561	302·994	75·749
3.	4·6730	2·7705	59·287	63·674	36·326	2279·751	303·184	75·796
4.	1·6320	0·9675	59·283	63·670	36·330	2279·920	303·354	75·838
5.	6·5976	3·9113	59·284	63·671	36·329	2279·890	303·325	75·831
	27·4334	16·2648	59·287	63·675	36·325	2279·711	304·145	75·786
				MALATE OF SILVER.				
1.	6·8730	4·2610	61·996	66·583	33·417	2180·141	303·575	75·894
2.	4·2635	2·6440	62·015	66·604	33·396	2179·490	302·924	75·731
3.	4·4305	2·7495	62·059	66·651	33·349	2177·951	301·385	75·346
4.	5·6490	3·5030	62·011	66·599	33·301	2179·621	303·054	75·764
5.	4·6820	2·9015	61·972	66·557	33·443	2181·011	304·444	76·111
	25·898	16·059	62·009	66·597	33·403	2179·707	303·141	75·785

It will be observed on first sight, with respect to the proportion of oxide and of acid in each particular salt, that our results yield the same number up to the third, and in many cases to the fourth figure; and, in our opinion, a better selection of salts could scarcely be made than that which was adopted, for the proportion of oxygen in these four salts is just as unequal as that of the hydrogen; hence if an error had been occasioned by the hydrogen, it could not possibly escape observation.

In the *acetic*, *malic*, *racemic*, and *tartaric* acids, the oxygen is as the numbers 3, 4, 5, and the hydrogen as 4 : 6.

With such coincidence as is exhibited in our analyses, the existence of any foreign admixture in these salts, which could have increased the atomic weight of carbon, cannot be supposed. We have, however, still another direct proof of their correctness, viz. in the results which Berzelius obtained in his analyses of the *tartrate*, and of the *racemate* of lead. These were as follows:—

Tartrate of Lead.

Nos.	Salt.	Oxide of Lead.	In 100 Parts.	
			Oxide of Lead.	Acid.
1.	2·000	1·25449	62·7245	37·2755
2	2·000	1·25434	62·7170	37·2830
3.	2·000	1·25522	62·7610	37·2390
4.	2·8873	1·81212	62·7618	37·2382
Mean	8·8873	5·57617	62·7431	37·2569

Nos.	Atomic Weight of Salt.	Sum of Four Atoms of Carbon.	Atomic Weight of Carbon.
1.	2223·191	303·743	75·936
2.	2223·460	304·012	76·003
3.	2221·904	302·456	75·614
4.	2221·871	302·423	75·606
Mean	2222·531	303·082	75·771

Racemate of Lead.

(Mean of several experiments.)

2·000 of the salt gave 1·2550 oxide of lead, from which we obtain 2222·290 as the atomic weight of the salt, 302·842 as the sum of the four atoms of carbon, and 75·711 as the atomic weight of carbon.

The results of our analyses of the acetates, tartrates, racemates, and malates of silver, give as the mean of five experiments with each, the following proportions:—

	Salt.	Silver.	Atomic Weights	
			of the Salt.	of Carbon.
Acetate of silver . .	28·803	18·612	2091·680	75·658
Tartrate	16·220	9·6175	2279·491	75·731
Racemate	27·4334	16·2648	2279·711	75·786
Malate	25·898	16·0590	2179·707	75·785

If we compare our experiments separately with one another, that is to say, the atomic weight of carbon as deduced from the one salt, with that from the others, we observe that the numbers obtained in the analyses of the same salt, agree more perfectly with one another, than do the mean numbers resulting from the analyses of the different silver salts.

If the experiments themselves be assumed to be perfectly exact, this discrepancy must, from the very nature of things, result from some cause. This cause can be no other than the difference between the specific gravities of the different salts. They were weighed, not *in vacuo*, but in air, and in the proportion of their unequal densities, they must displace unequal volumes of air. All these salts in the above weighings must lose weight, and those which are specifically lighter more than the specifically heavier salts. Dr. Clark, at the meeting of the British Association held at Birmingham in 1839, called attention to the influence which these relations exert on determinations by weight, and corrected the experiments of Berzelius accordingly. It is clear, that in weighing of from one to two grammes of substance, these differences will not affect the relation of numbers, but when 20 or more grammes are weighed, the correction ought not to be neglected.

Such a correction must result from a knowledge of the specific gravities of salts. We determined those of the four salts analysed by us, by taking a known weight of the salt in a saturated solution, and comparing the specific gravity of this liquid with that of pure water.

3·1281 gr. acetate of silver displaced 1 gr. = 1 c. c. water at 15° C. But 1 c. c. of air at that temperature weighs 0·00123 gr. (log. 0·0905137−2); 28·800 gr. acetate of silver displaces therefore 0·0113 gr. atmospheric air. These 28·803 gr. of salt weigh consequently 28·814 gr., but the weight was estimated by means of a brass weight of 28·803 gr., which having the sp. gr. 7·8, displaced 0·00455 gr. of air; that is to say, it lost that weight in air.

The above 28·814 gr. of acetate of silver, weighed therefore 28·8098 gr., and the 18·612 gr. of metallic silver which remains behind after the calcination weighed only 18·6113 grammes.

According to these corrections, 28·8098 gr. of the silver salt, yield 18·6113 gr. of metallic silver.

If we correct in this manner the atomic weights of the tartrate, racemate, and malate of silver, the specific gravities of which are *in vacuo*, respectively = 3·4321, 3·7752, and 4·0016, we obtain the following results:—

	Salt.	Silver.	Atom of Carbon.	Difference from the mean.
Acetate of silver ..	28·8098	18·6113	75·804	− 0·050
Tartrate of silver .	16·223	9·6171	75·861	+ 0·007
Racemate of silver	27·438	16·2641	75·908	+ 0·054
Malate of silver...	25·9019	16·0596	75·843	− 0·011
	Mean atomic weight of Carbon 75·854			

The discrepancies in the atomic numbers for carbon lie therefore in the fourth figure, or, what is the same thing, in the second decimal place. The atomic weight of carbon is therefore 75·854.

If we multiply the atomic weight of carbon as here given, viz. 75·854, by 1·1026, the specific gravity of oxygen, we obtain as the hypothetical weight of a volume of carbon vapour 0·83636; and if to this we add 2·20520, the weight of 2 volumes of oxygen, and divide the sum of these by 2, we obtain the number 1·521 for the specific gravity of carbonic acid.

The numbers which have been found for the specific gravity of carbonic acid, by means of the direct weighing of the gas, are—

	Sp. gr. of carb. acid.	Calculated at. weight.
By Biot and Arago	1·519	75·530
By Berzelius and Dulong	1·524	76·437
Calculated from our own analyses	1·521	75·854

We consider it as a further proof of the correctness of the atomic weight of carbon as determined by us, that our result lies intermediate between those of the observations of four such distinguished experimenters; and indeed all doubt must be dissipated, when we reflect that with our number the differences observed in organic analyses are legitimately accounted for*.

* Extract of a letter from Berzelius to Professor Wöhler:—

"April 2, 1841.

"Baron Wrede is engaged with experiments in reference to the specific gravity of gases. He has devised peculiar methods of obtaining a balloon of gas in a pure state, which supersede those employed by Dulong and myself. The weights of carbonic acid gas, as hitherto determined by him, nearly agree with those of Dulong and myself. He has found, however, that the specific gravity of that gas is not the same under unequal pressures, but that it diminishes with the decreased pressure until one-third of an atmosphere; below this point it is constant. Its specific gravity at the ordinary pressure

XXXII. *A new Method of Investigating the Resistance of the Air to an Oscillating Sphere. By the* Rev. J. Challis, *M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

I SHOULD have been unwilling to advert again to the subject of discussion between Mr. Airy and myself, unless it had appeared to me, after reading Mr. Airy's Reply in the August Number (p. 143), that something may yet be said in elucidation of this confessedly difficult question. Mr. Airy may feel assured that I have no fault to find with the manner in which he supports his own views, and that equally with him I am persuaded of the necessity of adhering to strictly legitimate principles in treating questions of this nature. By placing my reasoning under another point of view, I hope to show that it is not defective in this respect.

But first I must remark, that the argument by which Mr. Airy concludes that Poisson's solution is possible and mine impossible, is nothing to the purpose. I was fully aware that my solution would not satisfy the equation by which Mr. Airy has tried it; and it was scarcely necessary to prove that an integral obtained by so experienced a mathematician as Poisson satisfied the equation from which it was derived. The question really at issue is, whether that equation, without any limitation annexed, is the one which the conditions of the problem require. The reasoning I am about to adduce will, I think, prove that it is not, and at the same time will be an answer to all that Mr. Airy has said in the latter part of his Reply.

The following general proposition will be necessary for my purpose:—

If N *be a factor which makes* $u\,dx + v\,dy + w\,dz$ *an exact differential, then will*

$$N(u\,dx + v\,dy + w\,dz) = 0$$

be the differential equation of a surface which cuts at right angles the directions of the motion of the particles through which it passes.

The proof that follows is taken from that which Mr. Earn-

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is therefore somewhat too high, and it contains more than one volume of oxygen; thus it is clear that the atomic weight of carbon, as calculated from this, is also too high. That, calculated from his specific gravity, at the pressure of one-third of an atmosphere, is 75·7. He has as yet, however, only made three weighings, which he considers as little more than introductory experiments of practice. I shall be present tomorrow at the fourth determination."

* Communicated by the Author.

shaw has given of a very similar proposition in a memoir on Fluid Motion, contained in the Cambridge Philosophical Transactions (vol. vi. part ii. p. 204).

Let u, v, w be the resolved parts in the directions of the axes of rectangular coordinates, of the velocity V at a point of the surface whose coordinates are x, y, z; and let $x + dx$, $y + dy$, $z + dz$ be the coordinates of another point of the surface distant by ds from the former. Let the line ds joining the two points make angles α, β, γ with the axes of coordinates, and let the direction of the velocity V at the point xyz make the angles α', β', γ' with the same axes. Then

$$0 = \mathrm{N}(u\,dx + v\,dy + w\,dz)$$
$$= \mathrm{N}\,\mathrm{V}\,ds\left(\frac{u}{\mathrm{V}}\cdot\frac{dx}{ds} + \frac{v}{\mathrm{V}}\cdot\frac{dy}{ds} + \frac{w}{\mathrm{V}}\cdot\frac{dz}{ds}\right)$$
$$= \mathrm{N}\,\mathrm{V}\,ds\,(\cos\alpha\cos\alpha' + \cos\beta\cos\beta' + \cos\gamma\cos\gamma').$$

Consequently, as the factors without the brackets do not vanish, the quantity within the brackets, which is the cosine of the angle that the direction of motion makes with a tangent to the surface, must be equal to zero. Hence the motion is in the direction of a normal to the surface.

As the differential equation $\mathrm{N}(u\,dx + v\,dy + w\,dz) = 0$, may contain the time t in any manner, its integral will be of the form $\mathrm{F}(x, y, z, t) = \phi(t)$; or, differently expressed, $f(x, y, z, t) = 0$. For the sake of brevity I will call the surfaces given by this equation surfaces of *displacement*. In any proposed instance of motion there will be an unlimited number of such surfaces, each of which will in general be continually varying its form and position, as well by the change of t as by the change of form of the function f depending on the arbitrary circumstances of the motion. It will be a restricted case, but sufficient for our purpose, to consider the change of a given surface of displacement to depend only on the change of t, the function f retaining its form. Then the coordinates of the particle which at the time t were x, y, z, will at the time $t + dt$ be $x + u\,dt$, $y + v\,dt$, $z + w\,dt$. Consequently, as these are coordinates of the surface of displacement in its new position,

$$f(x + u\,dt,\ y + v\,dt,\ z + w\,dt,\ t + dt) = 0.$$

Hence taking the letter f to represent $f(x, y, z, t)$, we shall have

$$f + \frac{df}{dx}u\,dt + \frac{df}{dy}v\,dt + \frac{df}{dz}w\,dt + \frac{df}{dt}dt = 0.$$

But

$$f = 0;\quad \frac{df}{dx} = \mathrm{N}u;\quad \frac{df}{dy} = \mathrm{N}v;\quad \frac{df}{dz} = \mathrm{N}w.$$

Hence

$$N(u^2+v^2+w^2)+\frac{df}{dt}=0;\quad \text{or } N=-\frac{1}{V^2}\cdot\frac{df}{dt}.$$

This determination of the arbitrary factor N is the analytical proof that the assumed kind of motion is possible.

Let us suppose, for example, that the surface of displacement is a spherical surface, whose equation is

$$(x-a)^2+(y-b)^2+(z-c)^2=R^2,$$

R being its radius, and a, b, c the coordinates of its centre. And first, let a, b, c be constant, and R be a function of the time. Then

$$\frac{df}{dt}=-2R\cdot\frac{dR}{dt}=-2RV.$$

Hence $N=\frac{2R}{V}$, and consequently N is a function of the time.

Next let a, b and R be constant, and c variable. This is to suppose that the surface of displacement is a spherical surface of given radius, the centre of which is moving parallel to the axis of z. For this case we have

$$\frac{df}{dt}=-2(z-c)\frac{dc}{dt};\ \text{and } V=\frac{z-c}{R}\cdot\frac{dc}{dt}.$$

Hence $$N=\frac{2R^2}{(z-c)\frac{dc}{dt}}.$$

The supposed surface of displacement being thus shown to be possible, it necessarily follows, that if a smooth solid sphere move rectilinearly in the fluid, its surface coincides with a surface of displacement. Here then we have the case of the oscillating sphere, and as $z-c$ occurs in the expression for N, we have come to the important conclusion that $u\,dx+v\,dy+w\,dz$ is not an exact differential in this instance, when the variation of the coordinates is from one point to another of the surface of the sphere, and therefore not an exact differential for *every* variation of the coordinates. It becomes so when multiplied by $\frac{R}{z-c}$, if R be supposed to vary with x, y, z.

Let us, therefore, suppose in general that

$$N(u\,dx+v\,dy+w\,dz)=d\phi.$$

Then

$$u=\frac{1}{N}\cdot\frac{d\phi}{dx};\quad v=\frac{1}{N}\cdot\frac{d\phi}{dy};\quad w=\frac{1}{N}\cdot\frac{d\phi}{dz}.$$

Hence by substituting these values of u, v, and w in the equations (a), (b), (c), given in page 64 of the July Number, viz.

$$\frac{d\,\mathrm{P}}{d\,x} + k\frac{d\,u}{d\,t} = 0; \quad \frac{d\,\mathrm{P}}{d\,y} + k\frac{d\,v}{d\,t} = 0; \quad \frac{d\,\mathrm{P}}{d\,z} + k\frac{d\,w}{d\,t} = 0;$$

and neglecting quantities such as $\frac{d\,\mathrm{N}}{d\,t} \times \frac{d\,\phi}{d\,x}$, which will be of the order of the square of the velocity, it will be found that

$$\frac{d\,\mathrm{P}}{d\,t} + \frac{k}{\mathrm{N}} \cdot \frac{d\,\phi}{d\,t} = 0.$$

Hence from the equation

$$\frac{d\,\mathrm{P}}{d\,t} + \frac{d\,u}{d\,x} + \frac{d\,v}{d\,y} + \frac{d\,w}{d\,z} = 0,$$

it follows (since $k\,a^2 = 1$) that

$$\frac{d^2\,\phi}{d\,t^2} = a^2 \cdot \left\{\frac{d^2\,\phi}{d\,x^2} + \frac{d^2\,\phi}{d\,y^2} + \frac{d^2\,\phi}{d\,z^2}\right\}$$

$$-\frac{a^2}{\mathrm{N}} \cdot \left\{\frac{d\,\mathrm{N}}{d\,x} \cdot \frac{d\,\phi}{d\,x} + \frac{d\,\mathrm{N}}{d\,y} \cdot \frac{d\,\phi}{d\,y} + \frac{d\,\mathrm{N}}{d\,z} \cdot \frac{d\,\phi}{d\,z}\right\}.$$

Let us now transform this equation into one of polar coordinates r, θ, η, the angle θ being that which the radius vector r makes with the axis of z, and η that which the plane of these two lines makes with a vertical plane. And, guided by the foregoing result, let us assume the factor N to be $\frac{r}{z-c}$, or $\frac{1}{\cos\theta}$. By the usual rules of transformation I find that

$$\frac{d^2 \,.\, r\,\phi}{d\,t^2} = a^2 \left\{\frac{d^2 . r\,\phi}{d\,r^2} + \frac{d \,.\left(\sin 2\,\theta \frac{d \,.\, r\,\phi}{d\,\theta}\right)}{r^2 \sin 2\,\theta\,d\,\theta} + \frac{d^2 \,.\, r\,\phi}{r^2 \sin^2\theta\,d\,\eta^2}\right\},$$

an equation differing from Poisson's in having $\sin 2\,\theta$ in the place of $\sin\theta$ in the second term of the right-hand side, and also in the signification of ϕ. This equation, applied to the instance of the oscillating sphere, remains the same (small quantities being neglected) if the origin of the polar coordinates be the centre of the sphere. The resolved parts of the velocity in the directions of the three rectangular axes being

$$\frac{d\,\phi}{d\,x}\cos\theta, \qquad \frac{d\,\phi}{d\,y}\cos\theta, \qquad \frac{d\,\phi}{d\,z}\cos\theta,$$

the velocity in the direction of r will be $\frac{d\,\phi}{d\,r}\cos\theta$. Now the

conditions of the proposed problem require that when R is substituted for r in $\frac{d\phi}{dr}$, the equation

$$\frac{d\phi}{dr}\cos\theta = \frac{dc}{dt}\cos\theta$$

should be true, whatever be θ and η. Hence it follows that ϕ is a function of r and t only, and that $\frac{d\phi}{d\theta} = 0$, $\frac{d\phi}{d\eta} = 0$. The above equation consequently becomes

$$\frac{d^2 . r\phi}{dt^2} = a^2 . \frac{d^2 . r\phi}{dr^2},$$

according with what precedes both in giving for ϕ a function of r and t only, and in making $\sec\theta\,(u\,dx + v\,dy + w\,dz)$ an exact differential. Hence the value of the factor N has been correctly assumed. Since $\frac{d\phi}{d\theta} = 0$, and $\frac{d\phi}{d\eta} = 0$, the velocity is wholly in the direction of r, and $V = \frac{d\phi}{dr}\cos\theta$. But by integration, $r\phi = f(r - at)$. Hence

$$V = \left\{\frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}\right\}\cos\theta.$$

This is the value of V which I am contending for, and from it the result I originally obtained respecting the resistance of the air to an oscillating sphere necessarily follows.

All the above reasoning, excepting the introduction of the factor N, is according to the principles of the method adopted by Poisson. The sole reason of the difference of result is, that Poisson has argued as if $u\,dx + v\,dy + w\,dz$ were an exact differential in an instance, where, if my reasoning be good, that quantity is not an exact differential unless it be multiplied by a factor.

The investigation I have now gone through is in perfect agreement with the principles I advocated in my communication to the June Number of this Journal (S. 3. vol. xviii. p. 477), and may serve to place in a clear light the position I have there maintained, viz. that $u\,dx + v\,dy + w\,dz$ cannot in general be an exact differential unless the variation of the coordinates at a given instant be in the direction of a normal to a surface of displacement. This limitation is equivalent to the introduction of the factor N.

Being aware that I am arguing for a principle, which, if true, must have a very important bearing in the treatment of hydrodynamical problems, I feel it incumbent on me to support my

views by an accumulation of evidence, and will therefore add another argument. The equations (a), (b), (c), quoted above, are accurately true if the complete differential coefficients $\left(\frac{du}{dt}\right)$, $\left(\frac{dv}{dt}\right)$, $\left(\frac{dw}{dt}\right)$ be substituted for the partial differential coefficients $\frac{du}{dt}$, $\frac{dv}{dt}$, $\frac{dw}{dt}$, and become by this substitution

$$\frac{dP}{dx}+k\left(\frac{du}{dt}\right)=0;\ \frac{dP}{dy}+k\left(\frac{dv}{dt}\right)=0;\ \frac{dP}{dz}+k\left(\frac{dw}{dt}\right)=0.$$

Hence at the same time that $\frac{dP}{dx}dx+\frac{dP}{dy}dy+\frac{dP}{dz}dz$ is an exact differential, $\left(\frac{du}{dt}\right)dx+\left(\frac{dv}{dt}\right)dy+\left(\frac{dw}{dt}\right)dz$ is an exact differential. Let therefore this latter quantity be the differential with respect to x, y, z, of a function ψ of x, y, z and t. Then

$$\left(\frac{du}{dt}\right)=\frac{d\psi}{dx};\ \left(\frac{dv}{dt}\right)=\frac{d\psi}{dy};\ \left(\frac{dw}{dt}\right)=\frac{d\psi}{dz}.$$

Hence, by integration,

$$u=\int\frac{d\psi}{dx}dt;\quad v=\int\frac{d\psi}{dy}dt;\quad w=\int\frac{d\psi}{dz}dt.$$

Consequently, differentiating independently of the time,

$$\frac{du}{dy}=\int\frac{d^2\psi}{dy\,dx}dt=\int\frac{d^2\psi}{dx\,dy}dt=\frac{dv}{dx}.$$

So $\frac{du}{dz}=\frac{dw}{dx}$, and $\frac{dv}{dz}=\frac{dw}{dy}$.

Hence $u\,dx+v\,dy+w\,dz$ and $\frac{dP}{dx}dx+\frac{dP}{dy}dy+\frac{dP}{dz}dz$ are also exact differentials at the same time. Now any one arguing according to the views maintained by Mr. Airy would say that the latter quantity *is* unconditionally an exact differential; and how the conclusion could be avoided that $u\,dx + v\,dy + w\,dz$ is without limitation an exact differential in every instance of fluid motion whatever, I am unable to see. But this conclusion is certainly untrue. Poisson has written a long memoir on the propagation of motion in elastic fluids, on the supposition that that condition is not fulfilled. (See *Memoirs of the Paris Academy*, tom. x. 1831.) The explanation of this apparent contradiction I believe to be as follows:—When the complete differential (d P) is substituted for the sum

of the partial differentials $\frac{dP}{dx}dx$, $\frac{dP}{dy}dy$, $\frac{dP}{dz}dz$, it is tacitly assumed that P is a function of x, y, z, which varies *continuously*, and not arbitrarily, with the variation of the coordinates, and so far as the variation of the function is such, the substitution is legitimate. Now in any instance of fluid motion it is only in the direction of the *displacement* of the fluid that the variation of the velocity and density at a given time is in any respect necessarily continuous and not altogether dependent on the arbitrary displacement. In this direction it depends in part on the *propagation* of the motion, and on the mutual inclination to each other of the normals to the surface of displacement, in a manner which it is the peculiar province of the integration of a partial differential equation to make known, and so as to leave the particular form of the function expressing the arbitrary displacement indeterminate. But the variation of velocity and density at a given instant from one point to another *along* a surface of displacement altogether depends on the arbitrary disturbance, and cannot therefore be given by a similar process of integration. With the abovementioned limitation respecting the direction of variation of the coordinates, both $\frac{dP}{dx}dx + \frac{dP}{dy}dy + \frac{dP}{dy}dz$, and $u\,dx + v\,dy + w\,dz$ are exact differentials in every instance of fluid motion, excepting where the motion is not essentially different from that of a solid.

After this communication I shall not again enter upon the consideration of the problem which has given rise (not, I hope, without some advantage to the cause of science) to so prolonged a discussion. But the general principle on which my solution rests is too important to be hastily dismissed, and I shall probably take other opportunities of exemplifying it.

Cambridge Observatory, August 19, 1841.

XXXIII. *Note on the Production of Sulphuretted Hydrogen by the Action of decomposing Animal Matter upon Sulphates. By* Robert Mallet, *Ph. D., M.R.I.A.*

To Richard Phillips, Esq., &c. &c.

Sir,

THE valuable communication in the Number of the Philosophical Magazine for this month (July) by Mr. Daniell, on the spontaneous evolution of sulphuretted hydrogen in the waters of the African Coast, &c., has been read by me with peculiar interest, from its connexion with a subject to which

I have for some time devoted a good deal of attention, namely, the action of "air and water on iron," and other metals.

To Mr. Daniell must be awarded the merit of having first pointed out the very extended limits in which sulphuretted hydrogen thus occurs, and of indicating its highly probable connexion with pestilential diseases; but the claim which is apparently made in this paper (see page 11 *et passim*) by its author, to be the first who has pointed out the undoubtedly true source of this gas in the decomposition of sulphates by decomposing organic matter, cannot, I think, be sustained.

In the second report on the action of air and water, (whether fresh or salt) on iron, read at the last meeting of the British Association, and since published, section 171—172, the production of sulphuretted hydrogen in this way will be found fully pointed out, and its theory in the case of the iron salts even much more extended than had before been done; while the mere fact that the sulphates are deoxidized by organic matter in certain states has been observed years since, and even the formation of pyrites by the action of animal matter in putrefaction on the sulphates of iron; a case of which is, I think, recorded in Thompson's Annals of Philosophy. I have remarked that even insoluble sulphates, such as gypsum, can be decomposed in this way; but the power of decomposing either soluble or insoluble sulphates depends upon the *peculiar state* of decomposition in which the vegetable organic matter is found, and does not seem to me to take place, unless when carbonic acid is also present on the water, which it generally then is as a product.

About nine years ago the Directors of the Dublin and London Steam Marine Company requested me to examine the copper sheathing of their steam ship "Thames," since lost off "The Lizard," which had in a very short time corroded into holes, and on the ground of which they were about taking proceedings against the parties who supplied the copper. I found that the inside of much of the copper was lined with black sulphurates, such as Mr. Daniell describes; and on examining the "berth" where the vessel lay when in this port, I found it was over a bank of soft silt, and opposite a large sewer which constantly discharged putrid matter: to this I then attributed the decay of the copper, and advised the removal of the vessel to another berth, which was done after recoppering, and with the result desired: this, therefore, confirms the views on this subject Mr. Daniell has so well stated; but I feel myself compelled to differ with this gentleman as to the power of zinc *permanently* to protect copper from corrosion in sea-water thus charged with sulphuretted

hydrogen. The results of experiments made for very extended periods have shown me that unless the surface of the zinc be at intervals kept *clean* mechanically by scouring, &c., it soon ceases to protect, to any considerable amount, iron *or copper* in foul sea-water: its surface gets gradually covered with an impervious coating of microscopic crystals of calc spar, which diminish, and at length destroy all reaction upon it.

No experiments on the mutual protection of metals in water are worthy of dependence unless continued for a very long time, by which reactions are brought into evidence which would never have been perhaps suspected in experiments of much more limited duration.

Should you consider these observations suitable, I shall feel obliged by their insertion in the Philosophical Magazine, and am, Sir, your most obedient,

ROBERT MALLET.

94, Capel Street, Dublin,
July 4, 1841.

XXXIV. *Notices respecting New Books.*

Elements of Chemistry, including the most recent discoveries and applications of the Science to Medicine and Pharmacy, and to the Arts. By ROBERT KANE, M.D., M.R.I.A., Professor of Natural Philosophy to the Royal Dublin Society; Professor of Chemistry to the Apothecaries' Hall of Ireland, &c. Part II. Dublin, 1841, 8vo, pp. 357 to 676, with 49 wood-cuts.

HAVING in the preceding volume (S. 3. xviii.) of the Philosophical Magazine, p. 304, given an analysis of the first part of Dr. Kane's work, we now present our readers with the contents of the second part, in a similar form:—

Chapter X.—OF THE RELATIONS OF CHEMICAL CONSTITUTION TO THE MOLECULAR STRUCTURE OF BODIES.

Sect. I. *Of the Atomic Theory* (continued.)—Physical and chemical atoms. Various orders of molecular groups.

Sect. II. *Of Isomorphism.*—Relation of constitution to form. Various hypotheses on which the isomorphism of compound bodies has been explained. Crystalline forms of the simple bodies. Isomorphous groups. Principle of Plesiomorphism. Similarity of form does not prove necessary similarity of chemical constitution. Principles of isomorphous replacement. Of dimorphism and isomerism.

Sect. III. *Of Dimorphism and Isomerism, and of the Theory of Types.*—List of dimorphous bodies. Difference in structure of dimorphous bodies. Change of physical characters indicating an approach to dimorphism. Principle of isomerism. Connexion of dimorphism and isomerism. Possible isomerism of the metals. Nature of compound radicals. Constitution of organic bodies. Theory of organic types. Of actions by contact.

Sect. IV. *Of Catalysis.*—Of catalysis and analysis. Catalytic effects of heat. Catalytic effects ascribable to the communication of motion.

Chapter XI.—On the Classification of the Elementary Bodies. —Classification of bodies. Classification of elements.

Chapter XII.—Of the simple Non-metallic Bodies, and their Compounds with each other.—Of oxygen. Gasometers. Preparation of Oxygen. Its properties.

Of Hydrogen.—Its preparation. Its properties. Hydroxygen blowpipe. Hydrogen harmonicon. Chemical relation of hydrogen. Constitution of water. Solution of gases in water. Chemical relations of water. Various functions of water, in chemical combination. Mineral waters. Peroxide of hydrogen. Preparation and properties of peroxide of hydrogen.

Of Nitrogen.—Preparation and properties of nitrogen gas. Atmospheric air. Analysis of air. Composition of the atmosphere. Atmospheric air a mixture, and not a chemical compound. Constitution of the atmosphere. Law of diffusion of gases. Permanency of constitution of the atmosphere. Atmospheric pressure. Extent of the atmosphere. Poisson's atmospheric theory. Production of clouds, dew, &c. Compounds of nitrogen and oxygen. Nitrous oxide, its properties and composition. Preparation and properties of nitric oxide. Hyponitrous acid. Nitrous acid. Nitric acid. Natural and artificial production of nitric acid. Preparation of nitric acid. Properties and constitution of liquid nitric acid. Detection of nitric acid. Composition of nitric acid.

Sulphur.—Sources, properties and preparation of sulphur. Relations of oxygen to sulphur. Preparation and properties of sulphurous acid. Sulphurous acid. Sulphuric acid. Formation of sulphuric acid. Manufacture of oil of vitriol. Anhydrous sulphuric acid. Hydrates of sulphuric acid. Hyposulphurous acid. Hyposulphuric acid. Real constitution of the compounds of sulphur and oxygen. Preparation and properties of sulphuretted hydrogen.

Of Selenium, and its compounds with oxygen, hydrogen, &c.—Preparation and properties of *Phosphorus.* Compounds of phosphorus and oxygen. Phosphorous acid. Phosphoric acid. Phosphates of water. Salts of phosphoric acid. Preparation and properties of phosphuretted hydrogen.

Of Chlorine.—Preparation of chlorine. Bleaching power of chlorine. Oxidizing power of chlorine. Compounds of chlorine and oxygen. Hypochlorous acid. Chloric acid. Chlorous acid. Perchloric acid. Chloride of hydrogen. Composition of muriatic acid. Muriatic acid. Liquid muriatic acid. Nitro-muriatic acid. Chloride of sulphur. Chloride of phosphorus.

Iodine.—Preparation of iodine upon the great scale. Properties of iodine. Iodic acid. Feriodic acid. Hydriodic acid. Iodo-phosphuret of hydrogen.

Bromine.—Properties of bromine. Hydrobromic acid. Compounds of bromine.

Of Fluorine.—Hydrofluoric acid.

Of Silicon.—Of silica. Of silicic acid. Chloride of silicon. Fluoride of silicon. Hydrofluosilicic acid.

Boron and boracic acid.—Chloride of boron. Fluoride of boron. General characters of the metals.

Chaper XII. [XIII.]—Of the General Characters of the Metals, and of their Compounds with the Non-metallic Bodies.—Malleability and ductility. Compounds of the metals with oxygen. Classification of the metals. Degrees of oxidation. Relation of the metals to chlorine. State of the metals in nature. General principles of the reduction of the metals.

Chapter XIII. [XIV.]—Of the Individual Metals, and of their Compounds with Oxygen, Sulphur, Selenium, and Phosphorus.

Sect. I.—*Metals of the First Class.*

Of Potassium.—Preparation of potassium. Properties of potassium. Preparation of potash. Properties of potash. Sulphurets of potassium.

Of Sodium and soda.

Lithium and lithia.

Barium.—Compounds of barium. Preparation of barytes. Sulphuret of barium.

Strontium and strontia.

Of Calcium.—Compounds of calcium. State of lime in nature. Properties of lime. Sulphurets of calcium.

Of Magnesium.—Properties of magnesium. Properties of magnesia.

Of Aluminum.—Preparation of aluminum. Condition of aluminum in natural properties of alumina.

Sect. II.—*Metals of the Second Class.*

Of Glucinum and glucina.

Of Yttrium, Thorium, Zirconium, Cerium, Lanthanum.

Of Manganese.—Preparation of manganese. Oxides of manganese. Protoxide, sesquioxide, and peroxide of manganese. Determination of the real value of peroxide of manganese. Manganic and permanganic acids. Detection of manganese.

Sect. III.—*Metals of the Third Class.*

Of Iron.—State of iron in nature. Smelting of iron ores. Preparation of malleable iron. Manufacture of steel. Relations of iron to oxygen. Passive condition of iron. Various oxides of iron. Sulphurets of iron. Detection of iron.

Of Nickel.—Preparation of nickel. Nickel and its oxides.

Of Cobalt.—Preparation of cobalt. Cobalt and its oxides. Uses of cobalt in the arts.

Of Zinc.—Preparation and properties of zinc. Of the oxide and sulphuret of zinc.

Of Cadmium and its compounds.

Of Tin.—Extraction and properties of tin. Protoxide and peroxide of tin. Isomeric forms of stannic acid. Preparation of mosaic gold.

Of Chromium and its compounds.—Oxide and acid of chrome.

Of Vanadium.

Sect. IV.—*Metals of the Fourth Class..*

Tungsten and its oxides.—Compounds of tungstin.

Molybdenum and its compounds.

Osmium and its compounds.

Columbium and *Titanium.*—Titanic acid and titanic oxide.

Of Arsenic.—Properties of arsenic. Arsenious acid. Arseniuret of hydrogen. Sulphurets of arsenic. Detection of arsenic. Liquid tests of arsenic. Arseniuretted hydrogen. Marsh's tests for arsenic. Sources of fallacy. Arsenic naturally in the body. Antidote to arsenious acid.

Antimony and its oxides.—Oxygen acids of antimony. Sulphurets of antimony. Preparation of kermes mineral. Forms of sulphuret of antimony employed in pharmacy. Antimoniuret of hydrogen.

Tellurium and its compounds.

Uranium and its compounds.

Sect. V.—*Metals of the Fifth Class.*

Of Copper.—Reduction of copper from its ore. Properties of copper. Oxides of copper. Sulphurets of copper. Detection of copper. Useful alloys of copper.

Of Lead.—State of lead in nature. Oxides of lead. Sulphuret of lead.

Of Bismuth.—State of bismuth in nature. Atomic weight of bismuth. Compounds of bismuth.

Sect. VI.—*Metals of the Sixth Class.*

Of Silver.—Extraction of silver from its ores. Properties of silver. Oxides of silver. Detection of silver.

Of Mercury or Quicksilver.—State of mercury in nature. Properties of mercury. Oxides of mercury. Sulphurets of mercury. Detection of mercury.

Of Gold.—Extraction and properties of gold. Detection of gold.

Of Palladium.

Of Platinum.—State of platinum in nature. Different forms of platinum. Compounds of platinum. Detection of platinum.

Iridium and its compounds.

Rhodium and its compounds.

Chapter XIV. [XV.] ON THE GENERAL PROPERTIES AND CONSTITUTION OF SALTS.—General characters of salts.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xviii. p. 141 *.]

November 26 (*Continued*), 1840.—THE following papers were read:—

Description of a Percussion Shell to explode at the bottom of the Sea. By Captain J. Norton. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S., &c.

An iron tube, like the barrel of a musket, is screwed into a shell of any size, water-tight. A rod of iron, about half a pound in weight and a foot in length, is suspended within the tube, by means of a split quill passing through a hole in the upper end of the rod, the other end being armed with a percussion-cap. The mouth of the tube is closed with a screw lid also water-tight. Tin or brass wings being attached to the upper end of the tube will keep it in a vertical position during its descent to the bottom of the sea; and the shock on its striking the bottom will cause the bar of iron within the tube to fall, and produce the percussion and explosion.

Should it be found difficult to make the shell water-proof, I am satisfied that percussion powder made from silver will explode by friction or percussion even when *mixed with water*.

Memorandum addressed to the Royal Society. By T. Wharton Jones, F.R.S.

The following is the memorandum in the words of the author:—

On the 18th of June, 1835, a memoir, entitled, "On the Ova of Man and Mammiferous Animals, as they exist in the Ovaries before Impregnation, and on the discovery in them of a Vesicle analogous to that described by Professor Purkinje in the Immature Egg of the Bird †," was laid before the Royal Society.

[* The two abstracts of papers now inserted were accidentally omitted from their proper place; they are the notices referred to in vol. xviii., p. 547; and are followed in order of reading by those given at p. 307 of that volume.]

[† An Abstract of this memoir appeared in L. and E. Phil. Mag., vol. vii. p. 209.]

At the time I wrote, I believed myself the first who had observed the vesicle alluded to; but by a reference to the manuscript in the archives of the Society, it will be seen, from a postscript, that before sending it to be communicated, I had become aware that M. Coste of Paris had some time before announced that he had made a similar observation, as far as concerns the rabbit. Those who are conversant in such matters are doubtless aware that I was anticipated also by Professor Valentin: but of this circumstance I was not informed till some considerable time after.

It thus appears that, though I was an independent discoverer of the germinal vesicle of the mammiferous ovum, all the share in the discovery I can lay claim to *historically* is that of being the first who pointed it out in this country.

There is one point, however, in the anatomy of the germinal vesicle of the mammiferous ovum, of which I feel myself entitled to be recognized, especially by the Royal Society, as contemporaneous discoverer, and that is, the spot on the side of the vesicle. Feeling this, and having heard at the last meeting of the Royal Society the discovery of this spot attributed solely to the distinguished German physiologist, Professor Rudolph Wagner, I consider it due to the Royal Society and to myself to call to the Society's remembrance the fact, that, in the memoir above referred to as having been laid before them in 1835, the spot in question is not only pointed out and particularly delineated, but its physiological importance hinted at.

The laying of a paper before a Society is an act of publication. With the communication of my paper to the Royal Society in 1835, the publication of Professor Wagner's paper in Müller's *Archiv* was *contemporaneous* merely.

It is true, that though Professor Wagner's observations were only first published in Müller's *Archiv* for 1835, there is a note by the editor, saying that the paper was received by him in 1834; but it is also true,—and of this, were it necessary, proof could be easily adduced,—that my paper was written also in 1834.

In conclusion, I beg to apologize to the Royal Society for obtruding on their notice what may appear matter rather of personal than general interest.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xviii. p. 604.]

Nov. 13, 1840.—The following communications were read:—

A Letter from Mr. Dawes on the subject of a new Binary Star recently observed.

"I beg to call the attention of the Society to the star registered by Sir William Herschel as the 16th of his third class of double stars. Its Æ is $20^{h}\ 23^{m}{\cdot}6$, and N.P.D. 79° 17′. This star was measured by Herschel and South in 1822 with the five-foot achromatic. It was again observed by Struve on two nights in 1829, and also on two in 1832; and though powers of 320 and 480 were employed in the measurements, nothing remarkable was noticed by

him in either of the stars. On turning upon it Mr. Bishop's equatorially-mounted achromatic telescope, having an aperture of seven inches, and focus of nearly eleven feet, armed with a power of 320, though the character of the night (October 27) was very indifferent, I was immediately struck with the elongated appearance of the smaller of the two stars; and having applied higher powers, I procured measures of the direction of the elongation. I have since obtained two other sets with power 420, with which, in best moments, the elongated disc was slightly notched. The results of the three nights' observations are :—

October 27,	Position = 208°40′	Weight = 12
— 31,	... = 208 25	... = 11
November 4,	... = 208 44	... = 18
	Mean ...... = 208 38	

The estimated central distance = $0''\cdot6$ or $0''\cdot7$.

"It is extremely improbable that so acute an observer as Struve should have failed to recognise an appearance which is now so obvious and measurable with a much smaller instrument, if the star had then presented the same aspect as at present—the Dorpat refractor being capable of *distinctly separating* stars of the 8th or 9th magnitude whose central distance does not exceed $0''\cdot4$. Most likely, therefore, this star constitutes a new binary system; and it is highly desirable that, during the remainder of its present apparition, observations should be obtained by different individuals possessing instruments of sufficient optical power. To the notice of such, I beg earnestly to commend it.

"I may embrace this opportunity of announcing that the observations made during the last sixteen months at Mr. Bishop's Observatory afford satisfactory proof of binary character in several instances where it had been only suspected to exist, and of a very large amount of orbital motion in some binary systems previously known as such. The close pair of ε *Equulei* are decidedly more distant than when observed by Struve in 1835 and 1836. Within the last four years, 4 *Aquarii* has advanced 20° in its orbit; in which interval the star H i. 39 (Σ 3062) has changed its position to the extent of about 40°, and η *Coronæ* nearly 50°, with a central distance of scarcely $0''\cdot5$. The alteration is also striking in ζ *Herculis* (now measurable with a five-foot achromatic) and in Σ 2107; while τ *Ophiuchi*, which for years defied the power of the Dorpat telescope even to elongate it, has now opened out to the extent of nearly a second between the centres of its component stars. Supposing Sir W. Herschel's measure of the close pair of ζ *Cancri*, taken in 1781, to be exact, that remarkable binary system will now have *completed* a whole revolution since that date, that is, in 59 years.

"W. R. Dawes."

"Mr. Bishop's Observatory, Regent's Park,
Nov. 12, 1840."

A Supplemental Catalogue of the Right Ascensions of Fifty-five

Stars contained in the Royal Astronomical Society's Catalogue. By the Honourable John Wrottesley*.

Of the stars contained in this catalogue, seventeen had been wholly, or in part, observed at the time Mr. Wrottesley's former catalogue of 1318 stars†, for which the gold medal of the Society was awarded in 1839, was in course of observation, but are not included in that catalogue. The remaining thirty-eight are selected from the first list of stars accompanying Mr. Baily's "Address to Astronomical Observers," May 1837; and such were chosen as had not been previously observed at the author's observatory, or had not been observed at all since Piazzi's time, or presented discrepancies the clearing up of which seemed most likely to prove interesting. The observations of these thirty-eight were begun in May 1837, and carried on until August in the same year, by Mr. Hartnup; they were resumed by Mr. Wrottesley himself in December 1839, and concluded in August 1840. In every case the observations were continued until six or more of each star had been procured. In observing this catalogue all possible care was exercised that the mean places of the stars comprised in it should be entirely unaffected by any errors arising from imperfect instrumental adjustment. With respect to the standard stars employed, in 1837 only a few were used, sometimes only one, but care was taken that it should be situated very near the parallel of declination of the catalogue star. In 1840 all the Greenwich stars were used indifferently, that pass the meridian to the south of the zenith, and are comprised in the list of those with which the former catalogue was compared; and usually from five to six, and sometimes as many as from eight to ten, were observed on the same day, and the mean clock-error resulting therefrom used in deducing the catalogue stars. Bessel's right ascensions of these standard stars, and Mr. Wrottesley's own place of Fomalhaut, have been invariably used in reducing the catalogue stars; so that this catalogue, as well as the preceding, is founded on Bessel's mean places of the Greenwich stars. In an introduction to the catalogue, the author has explained in detail the different methods which he employed for determining the errors of level, collimation, and azimuth, and the other corrections applied in the reductions. The performance of the transit-clock during the progress of the observations was satisfactory; for though the rate was at times considerable in amount, it was always uniform. As a test of the extent to which the catalogue may be relied on for accuracy, Mr. Wrottesley states, that out of the forty-three stars contained in it, which have been observed by Mr. Airy, in no one case does the difference of the results, for a star more than 25° from the pole, exceed $0^{s}\cdot17$.

Postscript to Mr. Baily's Report on Mr. Maclear's Pendulum Experiments‡. By Mr. Baily.

* Now Lord Wrottesley, and President of the Society.

† See the Monthly Notices for November 1836; [or Phil. Mag., Third Series, vol. x. p. 227.]

‡ Read at the meeting of the Society in June 1840. [See vol. xviii. p. 602.]

The author states that the Admiralty having left to his decision the form and construction of a new pendulum, which they had resolved on sending out to the Cape, for the purpose of being swung by Mr. Maclear at the several stations of the trigonometrical survey now in progress in that colony, he had not hesitated in adopting the *bar*-pendulum, as by far the best and most convenient for a travelling instrument. The pendulum which has been accordingly constructed is a brass bar, sixty inches long, two inches wide, and about half an inch thick. It was formed of several thin plates which were pressed together by a rolling machine, and is, consequently, very compact and hard. Its specific gravity was 8·60, and its rate of expansion for one degree of Fahrenheit's thermometer, ·00001034. It is furnished with four knife-edges, thereby affording the advantages of four distinct pendulums on one and the same bar, and which thus serve as a check on each other. As the construction of the pendulum did not allow of much filing away at the ends without cutting into the knee-pieces, the vibrations on the several knife-edges were rendered nearly isochronous (for absolute isochronism can hardly be obtained), by fastening a circular piece of brass weighing 3000 grains, about an inch and a half from the centre of the bar; the weight and position having been determined by repeated preliminary experiments. After every thing was finished, seven sets of experiments were made on each knife-edge, the mean results of which were respectively as follows:—knife-edge A, 85906·322 vibrations; B, 85905·725; C, 85904·107; D, 85903·427; in a mean solar day. The computations and corrections were made in the usual manner, with the exception of the correction for the height of the barometer, which can only be determined accurately by swinging the pendulum *in vacuo*. For this there was not time before the pendulum was sent off, and the correction was assumed to be the *double* of that which is given by the formula which was usually employed prior to the experiments of M. Bessel. The agate planes, which were made expressly for this pendulum, are attached to a solid frame of brass, three-quarters of an inch thick, and having three foot-screws for the purpose of levelling the planes.

Observations of the Second Comet of 1840, made at the Observatory at Hamburgh. By Mr. Rumker.

The observations give the apparent right ascension and declination from January 29 to March 24, 1840.

Dec. 11.—The following communications were read:—

On a large Achromatic Object-Glass of a Telescope worked by Mr. Dollond, the flint glass of which was prepared by the late Dr. Ritchie. By the Rev. Samuel King, M.A., F.R.A.S.

In a paper by Mr. Simms, "On the Optical-Glass prepared by the late Dr. Ritchie," which was read to the Society on the 14th of June, 1839, and is now printed in vol. xi. of the Memoirs, reference was made to an object-glass of $7\frac{5}{8}$ inches aperture, the flint glass of which was worked by Mr. Dollond out of a disc prepared by Dr. Ritchie; and it was intimated that Mr. King, who had the object-glass at that time under trial, would probably report to the

Society upon its performance. Mr. King now states that the result of numerous observations on a variety of objects leads him to consider this glass as one of much excellence, though not faultless. There is scarcely any spherical aberration, and the light is very white and free from colour; but when the central portion is covered up, there is a good deal of irradiation, indicating a want of homogeneity near the edge of the lens, where the glass is very thick. For many objects, however, especially very faint nebulæ, the whole aperture may be used with great advantage; but for most purposes a contraction to six inches, or a little less, causes it to perform much better, and enables it to carry high powers with much distinctness. The powers tried with it vary from 40 to 700, and no doubt it would satisfactorily bear a considerable increase in this respect. It is not saying much of such an object-glass, that the small stars accompanying *Polaris*, *α Lyræ*, and *Rigel*, are readily seen; but it shows also with the greatest distinctness and completely separates the close stars of *ζ Cancri*, *ι* and *ζ Boötis*, *ξ Ursæ*, &c., and also *δ Cygni*, which affords one of the best tests of distinctness and perfection in a telescope. As a planetary glass, Mr. King remarks that he cannot speak of it so decidedly, not having had a fair opportunity of trying it. *Jupiter* and *Saturn* have both for a long time been at very low altitudes, and, with respect to his observatory, the entire range of each is through the smoke of London. But with the moon it performs magnificently, penetrating, as it would seem, into her very structure, when high magnifying powers are used. Upon the whole, he is of opinion that this object-glass will bear comparison with most others of the same size worked from the foreign material.

The object-glass is fitted into a brass cell, admitting of accurate adjustment by means of three screws with rods reaching to the eye end. The telescope, twelve feet in length, is mounted upon the rotative roof of a small observatory, in a manner which combines great ease in observing with freedom and steadiness of motion.

LONDON ELECTRICAL SOCIETY.

July 20, 1841.—The communications before the Society were,—1st, "On the Perforation of Non-conducting Substances by the Mechanical Action of the Electric Fluid." By Mr. Crosse.

Two wires were laid end to end and tied firmly on a piece of window glass, and a quick succession of sparks was passed between them by connexion with the respective conductors of a powerful machine; the result was a perforation. The same was varied in several ways; and was afterwards successfully tried with a crystal of quartz. The author concludes that even diamonds may be thus drilled.

2nd. "The Effects of Vegetable Points on Free Electricity." By Mr. Pine.

This was a continuation of the subject of a former paper, and contained some striking illustrations of the functions of leaves of growing plants, &c.

3rd. "An Account of Experiments undertaken to investigate the Nature of the Change of Colour of Bodies by Heat, and their conducting Power." By Mr. Pollock.

It is the author's object to show that the imponderable agents are all one. He has this in view in all the communications which he lays before the Society, and is indefatigable in gathering from every source such facts as may tend to bear on the one great object. How far he has been successful may be judged from a perusal of his several papers.

4th. "On the Method of restoring to weakened Magnets their primitive strength." By Professor Muncke. (Translation.)

The principle is simply to furnish the magnet with keepers in proportion to its strength, and to add more as its power increases.

5th. A Translation of an account of "Experiments made with a powerful Grove's Battery. By M. De la Rive."

Among the experiments was that of the rotation of the voltaic flame in obedience to the magnet; and the Professor's opinion on the subject is thus expressed in his own words: —"That which the magnet attracts or repels is that conductor formed of the series of particles of carbon transported from one pole to the other, and traversed by the current."

6th. The Secretary then laid before the Society Mr. Weekes's "Monthly Register for June," containing data upon several important points. This register will form a portion of the Society's Quarterly Part of the Proceedings.

Aug. 17.—The papers read this evening were,—1st, "On a New Electro-magnetic Machine." By Mr. B. Hill.

The polarities of the rotating magnets were so adjusted as to make available both the attractive and repulsive results, thus producing a power equal to the *sum* of these, whereas in other machines the power is only equal to the difference.

2nd. "Description of a smaller Atmospheric Electrical Apparatus." By W. H. Weekes, Esq.

The insulating apparatus in this is similar to that in the larger apparatus already described in the Proceedings of the Society; the collecting arrangement is a *pointed wire*, in place of 365 yards of wire. The use of such an apparatus is very limited, and not to be depended on except for certain purposes. Mr. Weekes mentions, that a lighted candle, in a metal candlestick insulated on wax, will gather indications of electricity when other means fail.

3rd. "Further Observations on Electrotype Manipulation:—Fusible Metal." By Charles V. Walker, Esq., Hon. Sec.

The author here describes the composition of fusible alloy, and the mode of producing *Clichée* medals,—fusible casts from fusible moulds. The method is not so generally known here as on the Continent; it is very simple and effectual. Specimens were on the

table obtained by these means, for which the author expressed himself as indebted to Mons. C. de Rheims of Calais.

4th. "Methods of giving more force and stability to the Currents of Galvanic Batteries formed by a single Liquid." By Professor Poggendorff. (Translation.)

The copper plates of batteries are submitted to one of four processes, which gives them a surface analogous to that of Mr. Smee's platinized platinum. One of the methods is to deposit copper on them by electrolysis. The action of such plates in combination with amalgamated zinc is both powerful and constant.

5th. "On a Voltaic Process for Etching Daguerréotype Plates." By W. R. Grove, Esq., M.A., F.R.S., Prof. Exp. Phil. Lond. Inst.

This discovery enables us to multiply in a durable material the fleeting and delicate traces of Daguerréotype. The plate to be etched is made a positive electrode, in an electrolyte of dilute hydrochloric acid; and the action is continued for a few seconds. The etchings are fit for the printer. Several prints obtained from plates thus prepared were placed before the Society, and were much admired as specimens of what may be done when the art has been further carried out. The author states that these prints are not so true to nature as the original picture, because in order to etch deep enough to receive the printing ink, some of the fine lines will blend. This will not practically be an objection to the process, because no lines are lost except those which, if present, could not be appreciated. One very important application of the art is to etch very delicately a picture, and to take from this *perfect* etching electrotype copies. These are so true that the author actually read on one, by microscopic aid, five lines of inscription on a surface 1-10th by 6-100dths of an inch. The following are the concluding words of this communication:—"I transmit with this paper some specimens of engravings of the etched plates, and of electrotypes taken from them; and in conclusion would call attention to the remarkable instance which these offer of the effects of the imponderable upon the ponderable. Thus, instead of a plate being inscribed as 'drawn by Landseer and engraved by Cousins,' it would be 'drawn by Light and engraved by Electricity.'"

The business of the evening terminated by the reading of Mr. Weekes's Monthly Journal of the electric state of the atmosphere.

ROYAL IRISH ACADEMY.

Abstract of Observations on the Origin of Audible Sounds. By Robert Kane, *M.D.**

Nov. 30, 1840.—Dr. Kane read a paper "On the Production of Audible Sounds," of which the following is an abstract.

The sensation of sound is produced upon the ear by the tympanum being thrown into vibratory motion, isochronous with the vibrations transmitted from the sounding body.

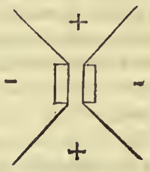
* From the Proceedings of the Royal Irish Academy.

Any body which vibrates as a single mass gives origin at the same moment to two waves, whose motions are in opposite directions, and of which one is rarefied and the other condensed.

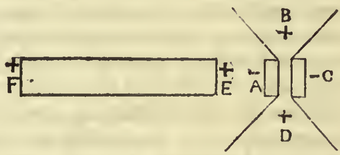
If these two arrive at the tympanum at the same moment and with equal power, perfect neutralization should result, and no sound be heard: hence, where a vibratory body produces upon the ear the sensation of sound, it arises from one wave of the two being either totally intercepted, or, at least, diminished in force, and the loudness of the sound is proportional to the difference of the intensity of the two waves when they affect the ear.

All instruments for increasing sound, and producing resonance, act upon this principle.

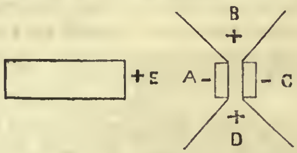
The following facts will illustrate these principles in detail. A tuning fork is a centre of four waves, two + and two —, but unless it be very close to the ear, no sound is heard from it; because the centre of all the four waves being very close, all act on the ear with equal force, and the difference is 0 (approximately).



Now, if an open tube, of the same length as a one-phase wave from the fork, be approached to one centre, as A, in the adjoining figure, the air in it commences to vibrate in unison with the fork, from being set in motion by the first wave which passes into it: the vibration of the tube is, however, a phase behind that of the fork, and hence, when a — wave passes from the centre A, it meets a + wave from the end of the tube E, and both are destroyed. The —centre, C, destroys also a + centre, as D, and there remain only the centres of + waves, B from the fork, and F from the tube, and these acting in concert on the tympanum produce the sound that we hear.

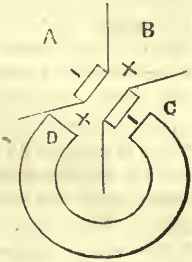


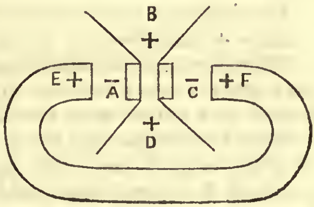
If the tube be closed, and of only one-half the length, the + wave, which emanates from the centre A, passes in, and being reflected from the bottom, issues again at the moment when the next — wave from A is about to enter; E and A then destroy each other, and C and D also interfering, there results only the + wave B, which acts unimpeded on the ear. The sound of an open tube is, therefore, *ceteris paribus*, much stronger than that of a closed tube, as there are two waves in place of one.

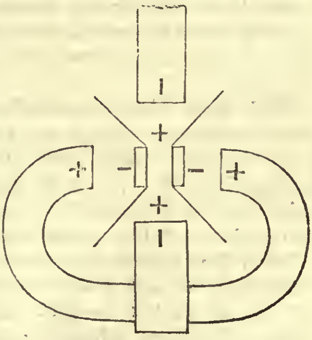


That the office of closed tubes, when resonant, is to destroy a portion of the sound of the original vibrating body, and of the open tubes to afford, in addition to that, a new centre of a wave of the same phase as that which remains, may be exhibited in many ways.

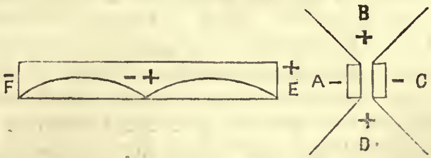
Thus, Mr. Adams showed long since, that when two closed tubes are placed at right angles to each other, they interfere when made to speak to a tuning fork, and for this effect no explanation has hitherto been given. But it is evident that the tubes being at right angles to each other, the waves destroyed are in opposite phases, and those which remain are in opposite phases also, so that the effect is the same as if no tubes were present at all. The same effect may be produced by a single tube, bent so that its apertures may be at right angles to each other; the — and + waves, D and C, meeting in the tube, produce neutralization, and the waves A and B, also + and —, which remain, interfere also, and hence no sound results. In an open tube bent into a circle, as in the figure, the two waves destroyed (A C) are of the same phase, and also those which remain (B D), and hence such a tube sounds with nearly double the power of an ordinary open tube. That it is the sound of the waves which do not go into the tube, and not that of the waves in the tube, we hear, may be shown by applying two closed tubes, as in the next figure. When the two — waves are absorbed by the circular open tube, each closed tube absorbs a + wave, and hence, notwithstanding that there is so much vibrating material, no sound is heard. But if the tubes A and B were open, then the vibrating centres should have been simply transferred to their further extremities, and the tubes would emit sound as the fork had done without them in the preceding figure.







If the open tube be double the length of a phase, then the neutralization occurs as in the figure, the residual waves being B and F, in opposite phases; but as their centres are separated so far, they interfere only in hyperboloidal planes, which are not detected unless when carefully sought for, but have been noticed to exist by Savart, although he did not suspect their cause.



All these principles have received very full verification from an instrument constructed for the purpose, and termed a *Chorizophone.* It consists of a square glass plate, which is placed above a set of closed tubes of such size, that when the plate vibrates in four pieces, with diagonal nodal lines, the length of each tube is half the length of the phase of the wave produced, and their form is triangular, of the magnitude of one of the four vibrating portions of the plate; when one of these tubes is presented to the plate, and this brought to vibrate by a violin bow applied to the centre of one of the sides, the tube resounds, and more loudly in proportion as the plate is brought nearer to its orifice. Now here the entire wave from the plate is caught by the tube, and the more perfectly its escape into the air is prevented, the louder is the sound produced, the sound must arise therefore from the waves which do not pass into the tube. Any one or more waves may thus be absorbed by the closed tubes, and a range of loudness of sound produced from the same plate with one or more of the four tubes, according as they are disposed as follows:—

The vibrating plate gives eight waves, four above and four below, 4 being + and 4 —.

With one tube, one wave is absorbed, and 3 + and 3 — destroying each other, a wave remains opposite in phase to that which is absorbed, and produces an audible sound.

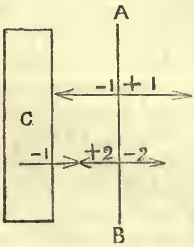
With two tubes, the waves absorbed may be either of opposite or of the same phases. If opposite, then the remaining waves are 3 + and 3 —, and no sound is produced; but if the waves absorbed be of the same phase as +, then there remains 4 — and 2 +, and hence the ear is doubly affected by 2 —. The two tubes may be either both above or both below, or one above and one below the plate.

With three tubes, the absorbed waves may be either all of the same phase, or two of one and one of the other. In the first instance, 3 + being absorbed, there remains 4 — and 1 —, and the ear receives the impulse of 3 —. In the other case 2 + and 1 — being absorbed, there remains 2 + and 3 —, and the impulse on the ear is only 1 —. The position of the tubes may vary in this as in the former case.

With four tubes, the absorption may be either all of the same phase, or 2 + and 2 —. In the former case, the remaining waves will be either 4 + or 4 —, in which case the greatest sound the plate can produce is heard, or else there remain 2 + and 2 —, in which case the plate gives no sound. These results prove fully that it is the residual sound that is heard, and not that which passes into the tube.

A vibrating plate gives some sound always, even without the tubes, for since there are at least eight waves, some one will always be more favourably disposed for acting on the ear than another: this difference will increase with the number of waves; and hence the independent sound of a plate increases in proportion as the vibrating portions into which it divides become more numerous.

A string vibrating in free space produces little or no sound; but if it be strung over, or in connexion with, an elastic board or box, a great resonance is produced. This arises from two sources; first, the string when by itself is the centre of two waves excessively close, and the action of which is therefore interfering. But if the string A B vibrate near a plane surface C, the wave − 1, which passes towards it, is reflected back, and meeting the wave + 2, which follows, it neutralizes it partly, and enables the wave − 2 to reach the ear without diminution. It is probable, however, that the great portion of the sound arises from the board or plate itself vibrating in parts, or as a whole. If in parts, these parts are variously situated, as regards the ear, and hence produce an effect upon it. Or if, as a whole, the plate C is so broad, or bounded, if a box, that one wave is lost by internal reflexion, and only the wave emanating from the outer surface can arrive at the ear.



When a tuning fork is placed on a table, one wave is lost by internal transmission and reflexions, whilst that directed from the outer surface reaches to the ear.

In the case of reed instruments, the reed produces two waves, which, if it vibrated freely, should neutralize each other on the ear; but in practice whilst an open passage is allowed to one by the mouth-piece, the other wave is lost within the cavities of the lips and mouth. In mouth-piece instruments, as bugles and trumpets, the cavity of the mouth serves also for the absorption of the one wave, leaving the other free to act.

XXXVI. *Intelligence and Miscellaneous Articles.*

ACTION OF SULPHURIC ACID ON NITRATE OF AMMONIA. BY M. PELOUZE.

WHEN sulphuric acid and nitrate of ammonia are mixed at common temperatures, no phænomena occur which might not be expected, whatever may be the properties employed, that is to say, reagents indicate the presence of ammonia, sulphuric acid and nitric acid.

If the mixture contain water and be submitted to distillation, nitric acid and the sulphate of ammonia indicated by theory are obtained. When, on the contrary, the nitric of ammonia is deprived of water, and it is heated in fifty times its weight of concentrated sulphuric acid, the mixture yields, at about 302° Fahrenheit, a very considerable quantity of nitrous oxide; water is formed, which combines with the sulphuric acid, and neither nitric acid nor ammonia exists in the product of this reaction. The nitrate of ammonia behaves under these circumstances as it does when merely heated, and

it affords the only example of a nitrate which does not yield nitric acid by the action of sulphuric acid, and at the same time give up its base to this acid, more fixed and energetic than nitric acid.

When the proportion of sulphuric acid is most diminished, operating, for example, with ten parts to one of nitrate of ammonia, about three-fourths of the salt are decomposed into nitric acid and ammonia, and one-fourth into nitrous oxide and water. By gradually diminishing the proportion of sulphuric acid, but little nitrous oxide is obtained, so that with one equivalent of nitrate of ammonia and two equivalents of sulphuric acid,the phænomena coincide with those of the decomposition of a salt by a more fixed acid.

When a mixture of nitrate of ammonia and a large excess of sulphuric acid is heated only between about 200° and 250° Fahr. instead of 322° Fahr., nitric acid distils, but without the production of any nitrous oxide. When nitrite of ammonia is heated with a great proportion of concentrated sulphuric acid, it is converted as by the mere action of heat into azote and water.

It appears from the preceding statements, that very different results are obtained by varying the proportions of nitrate of ammonia, sulphuric acid, the quantity of water and the temperature employed. —*Journal de Pharmacie*, t. xxvii. p. 271.

PREPARATION OF AZOTIC GAS. BY M. PELOUZE.

M. A. Rose has observed that monohydrated sulphuric acid combines directly with nitric oxide, and absorbs very considerable quantities of it. Sulphate of ammonia being heated in this compound to about 322° Fahr., pure azote gas, perfectly unmixed nitrous or nitric oxide, was obtained. This experiment was varied by passing nitric oxide gas into concentrated sulphuric acid mixed with sulphate of ammonia, and heated from about 300° Fahr. to 392°. The nitric oxide was decomposed as in the preceding experiment, and pure azotic gas was obtained; it is mixed with nitric oxide only when the disengagement is too rapid. M. Pelouze is of opinion that this method of preparing azotic gas may be advantageously employed.

CONCENTRATION OF NITRIC ACID BY MEANS OF SULPHURIC ACID. BY M. PELOUZE.

It is stated in different chemical treatises, that concentrated sulphuric acid decomposes nitric acid into water, with which it combines, and hyponitrous acid. M. Pelouze doubted the accuracy of this statement, and was convinced that it was erroneous, by observing nitric acid distil from a mixture of nitrate of ammonia with great excess of sulphuric acid at 212°.

500 parts of concentrated sulphuric acid were mixed with 100 of nitric acid of specific gravity 1·448; the mixture was slowly distilled, and yielded 88 parts of nitric acid of specific gravity 1·520; this product, freed from red vapour by a gentle heat, was mixed with six and a half times its weight of concentrated sulphuric acid, un-

accompanied with any sensible increase of temperature. The mixture was colourless, and yielded very dense white vapours of nitric acid. When heated to a temperature which never exceeded 302°, and was kept as near as possible to 212°, 82 parts of nitric acid of specific gravity 1·520 were distilled; its density remained 1·520, and its boiling point was from 185° to 188° Fahrenheit.

A third rectification with sulphuric acid effected no change either in the properties, density or colour of the nitric acid.—*Journal de Pharmacie*, t. xxvii. p. 275.

COMPOSITION OF SUGAR OF GELATIN.

M. Boussingault observes, that the existence of sugar of gelatin discovered by M. Braconnot had been questioned by several chemists. It was obtained by him by acting on glue with sulphuric acid; and on following his directions M. Boussingault obtained the sugar in question, and leucin. The composition and constitution of this sugar, according to M. Boussingault, are,—

	By experiment.	By calculation.	
Hydrogen	6·44	6·36	H^{36}
Carbon	33·85	34·	C^{32}
Oxygen..........	39·71	39·59	O^{14}
Azote	20·	20·05	N^{8}
	100·00	100·00	

Sugar of gelatin is readily combined with oxide of silver; the compound forms colourless crystals, which are but slightly soluble in cold water. They consist of

Hydrogen	1·21	
Carbon................	13·66	
Oxygen	12·31	
Azote	8·07	
Silver	63·95	99·2

Sugar of gelatin combines also with the oxides of copper and of lead; these two compounds are very soluble in water; the cupreous compound is a crystalline mass of an azure blue colour; that of oxide of lead crystallizes in fine colourless needles; its solution is totally decomposable by carbonic acid. The composition of both these compounds is analogous to that of the silver compound.—*Journal de Pharmacie*, t. xxvii. p. 35.

LACTATE OF UREA IN URINE.

MM. Cap and Henry some time since expressed it as their opinion that urea existed in urine, combined with lactic acid, and probably also the phosphoric; the correctness of these opinions having been questioned by M. Lecanu, the authors have again investigated the subject, and they are of opinion that the following experiments demonstrate the accuracy of their statement.

When fresh human urine is evaporated to about five-sixths of its bulk, at a temperature not exceeding 248° Fahr., there is obtained,

on cooling, a bright, brownish, very acid liquor, which must be carefully filtered to deprive it of a deposit of a dirty white colour, formed during concentration. The liquid is again evaporated, with a gentle heat, to the consistence of a syrup, and then nearly to dryness, *in vacuo*, over some substance powerfully attractive of water. The residue is to be put into a stoppered glass bottle, and there are to be added to it cold, ten or twelve times its weight, of a mixture of two parts of sulphuric æther and one part of rectified alcohol; it is to be often shaken, and, after some days, the liquid, which has acquired an amber colour, is to be poured off; this liquid is very acid, and is to be shaken in a bottle, with a slight excess either of the carbonate of lime, of zinc, or of barytes, or bicarbonate of potash. From their reaction effervescence results, and there are produced lactate and phosphate of lime, zinc, barytes, or potash, by the saturation of the *free* lactic and phosphoric acid, which the urine contains.

The liquid æthereal portion, again filtered and exposed to a very gentle heat, soon yields in both cases, very fine prismatic crystals of lactate of urea, perfectly similar to those artificially prepared. These crystals are long transparent hexagonal prisms, have a sharpish taste, are volatile at a moderate heat, entirely decomposed when heated to redness on platina foil, very soluble in water, alcohol, and alcoholized æther, but less so in sulphuric æther. They attract moisture from the air powerfully, dissolve entirely in it, and form a bright coloured solution, which, when gently heated, again furnishes crystals. Oxalic and nitric acids form, when added to the solution, either laminated crystalline precipitates, or pearly acicular crystals. Hydrate of lime does not disengage ammonia, as immediately occurs with ammoniacal salts. In order to determine the state of combination of the lactic acid with the urea, the following experiments were made:—A quantity of the crystals obtained by the spontaneous evaporation of alcoholized æthereal liquor, were pressed on filtering paper, and divided into three equal portions, A, B, C.

A. These crystals, dissolved in pure water, were slightly heated with an excess of hydrate of zinc, recently prepared and dried by exposure to the air. The mixture was carefully evaporated to dryness, and treated with hydrated sulphuric æther, which dissolved the urea, and yielded it, after evaporation in the air, in crystals which were not hygrometric. The residue, not acted upon by this, was treated with hot distilled water, filtered and evaporated on a sand bath. This operation yielded white acicular styptic crystals of lactate of zinc.

B. Another portion of the crystals was treated with a solution of barytes; it was evaporated to dryness, and treated successively with sulphuric æther and weak alcohol. The æther dissolved the pure urea, and the alcohol dissolved the dry lactate of barytes; it was very soluble, decomposable by heat, and precipitated sulphates like other barytic salts.

C. Lastly, to the third portion of the crystals, dissolved in a small quantity of water, oxalic acid was added till the crystalline precipitation of oxalate of urea ceased. The solution was carefully evapo-

rated to dryness, and the product was subjected to the action of æther mixed with a fifth of rectified alcohol.

The liquid, treated with excess of carbonate of lime, was left in contact in the cold with alcohol. Hot distilled water was afterwards shaken with the undissolved residue, and after having been filtered, evaporation was carefully conducted. The product of this last operation was a soluble salt, which crystallized on standing, and possessed all the well-known characters of lactate of lime. If excess of nitric acid be used instead of oxalic acid, oxalate of lime is obtained instead of lactate, for the nitric acid converts the lactic into oxalic acid.

MM. Cap and Henry observe, that it appears to them evidently to result from the preceding experiments, that after having separated the microcosmic salt from the evaporated urine, and saturated the lactic and phosphoric acid in excess, and having obtained a crystallized product by the intervention of alcoholized æther, in which product M. Lecanu admitted the presence of lactic acid and urea; that these crystals have the form and characters very distinct from those which belong to pure urea, and that they perfectly resemble those procured by the direct action of lactic acid on urea; lastly, that the hydrates of zinc, barytes, lime and oxalic acid, occasion in these crystals the production of lactates of zinc, barytes and lime, and separate lactic acid; they observe that it is impossible to demonstrate in a more direct manner the existence of a compound of these two principles.—*Journal de Pharmacie*, xxvii. 356.

METEOROLOGICAL OBSERVATIONS FOR JULY 1841.

Chiswick.—July 1. Overcast. 2. Hazy: overcast and fine: slight rain. 3. Hazy and mild: very fine. 4, 5. Very fine. 6. Rain: fine. 7. Fine: rain. 8. Fine: clear. 9. Very fine. 10. Fine: rain. 11. Overcast. 12. Cloudy and mild. 13. Cloudy. 14. Showery. 15. Heavy thunder-showers: constant heavy rain. 16. Cloudy. 17, 18. Fine. 19. Very fine. 20. Heavy rain. 21. Overcast: rain. 22. Cloudy. 23. Cloudy and fine. 24, 25. Cloudy. 26, 27. Light haze: fine. 28. Cloudy. 29. Cold and dry: cloudy: slight rain. 30. Fine but cool. 31. Rain: cloudy.—The quantity of rain which fell on the 15th was unusually great, amounting to nearly an inch and a half in the course of the twenty-four hours.

Boston.—July 1. Rain. 2. Cloudy. 3. Fine. 4. Cloudy: rain P.M. 5. Cloudy. 6. Rain and stormy: rain early A.M.: rain P.M. 7. Cloudy: rain P.M. 8. Fine. 9. Cloudy: rain P.M. 10. Fine: rain early A.M. 11. Cloudy. 12. Fine: rain P.M. 13. Cloudy: rain P.M. 14. Fine. 15, 16. Fine: rain P.M. 17. Fine. 18. Cloudy: rain P.M. 19. Fine: rain P.M. 20. Rain: rainy day. 21. Cloudy: rain P.M. 22. Cloudy: rain P.M., with thunder and lightning. 23. Cloudy: rain P.M. 24—28. Cloudy. 29. Cloudy and stormy: rain A.M. and P.M. 30. Fine. 31. Cloudy: thunder and lightning P.M.

Applegarth Manse, Dumfries-shire.—July 1. Fine: one shower. 2. Fine and fair, but cloudy. 3. Sunshine and rain. 4. Slight showers. 5. Rain all day. 6. Fair and fine. 7. Cloudy A.M.: fine P.M. 8. Fine: one shower. 9. The same. 10. Wet afternoon. 11. Slight showers. 12. Slight showers, but heavier. 13. Heavy showers: thunder. 14. Incessant showers. 15. Heavy rain till noon. 16. Fair and fine. 17. Fair but threatening. 18. Fair and warm: thunder. 19. Fine with a few drops. 20, 21. Rain P.M. 22. Slight showers. 23. Cloudy but fair. 24. Fine all day. 25. Remarkably fine. 26. Showers. 27. Showers P.M. 28. One slight shower. 29. Fair throughout. 30. Fair but threatening. 31. Fair with a few drops.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. July.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
1.		30·080	30·049	29·47	29·76	29·84				63	59	62·5	64	51		SW.	W.	W.		·05	·22	...	
2.		30·123	30·086	29·52	29·98	30·01				69	60	63·5	65	53½		W.	calm	E.		·01	·06	...	
○ 3.		30·126	29·989	29·58	29·95	29·82				79	55	67	64	56		SW.	calm	SSW.		·04	...	...	
4.		29·999	29·963	29·37	29·80	29·95				69	53	66·5	64	55		SW.	calm	E.		...	...	...	
5.		30·064	29·929	29·55	29·97	29·65				75	57	61	59	48½		NE.	calm	SE & S.		·59	·03	...	
6.		29·805	29·635	29·03	29·58	29·68				72	52	60	64	52		W.	W.	NW.		·03	1·27	...	
7.		29·885	29·639	29·31	29·65	29·55				67	53	61	59	50		SW.	W.	W.		·08	·03	0·78	
8.		29·898	29·742	29·20	29·63	29·70				67	45	58	60½	47½		W.	NW.	W.		·01	·16	...	
9.		29·949	29·939	29·44	29·75	29·79				71	44	60	57	45½		W.	W.	WNW.		...	...	...	
10.		29·926	29·492	29·44	29·69	29·20				71	49	58	55½	45		SW.	calm	SW.		·36	·09	...	
☾ 11.		29·572	29·266	28·77	29·27	29·39				70	43	57·5	59½	47½		NW.	calm	NNE.		...	·26	...	
12.		29·594	29·578	29·05	29·43	29·46				64	44	58	40	45		W.	W.	NNE.		·02	...	...	
13.		29·719	29·630	29·13	29·49	29·55				68	42	58	59	48		W.	calm	NW.		·02	·06	...	
14.		29·726	29·570	29·25	29·56	29·57				66	45	60	57½	50		SW.	W.	NW.		·07	·04	1·80	
15.		29·742	29·646	29·16	29·58	29·71				68	51	60	61	48½		S.	calm	NE.		1·46	...	...	
16.		29·977	29·874	29·40	29·86	29·87				72	44	61	61	47½		N.	calm	NE.		·01	·03	...	
17.		30·013	29·820	29·53	29·85	29·75				75	49	63	63	40½		S.	calm	SSW.		...	·17	...	
● 18.		29·742	29·634	29·24	29·68	29·70				75	48	64	67	49		NW.	calm	W.		...	...	...	
19.		29·813	29·795	29·30	29·69	29·65				73	54	62	66	51½		W.	calm	SW.		·07	·01	...	
20.		29·699	29·493	29·15	29·49	29·30				63	55	59	64	52		SW.	W.	ESE.		·29	·09	...	
21.		29·665	29·469	28·92	29·33	29·43				67	54	63·5	64	53		SW.	W.	NW.		·10	·28	0·49	
22.		29·828	29·724	29·16	29·55	29·70				67	51	61	58½	48½		W.	W.	NNW.		·22	·14	...	
23.		29·999	29·915	29·39	29·82	29·96				64	53	56	62	49		NW.	W.	E.		·01	·65	...	
24.		30·133	30·085	29·66	30·05	30·08				60	50	55	62	51		NE.	NW.	NW.		...	·02	...	
☽ 25.		30·127	30·071	29·65	30·06	30·00				68	51	58	62	51		NE.	calm	W.		...	...	...	
26.		30·074	30·022	29·56	29·98	29·94				72	53	61	66	51		SW.	calm	SW.		...	...	...	
27.		29·990	29·943	29·45	29·85	29·62				70	49	58·5	55	47		NW.	NW.	SW.		...	...	...	
28.		29·839	29·774	29·20	29·53	29·55				72	47	61	60	51½		SW.	W.	W.		...	...	0·05	
29.		29·760	29·722	29·09	29·46	29·48				65	48	55	58½	48		W.	W.	W.		·01	...	...	
30.		29·695	29·621	29·10	29·49	29·40				63	48	58	56	45½		NW.	W.	WNW.		...	·13	...	
31.		29·622	29·576	29·03	29·35	29·54				62	44	54	60	48½		W.	W.	NW.		·11	...	...	
Mean.		29·877	29·764	29·29	29·681	29·682				68·61	50·00	60	61	49·1						3·56	3·74	3·32	

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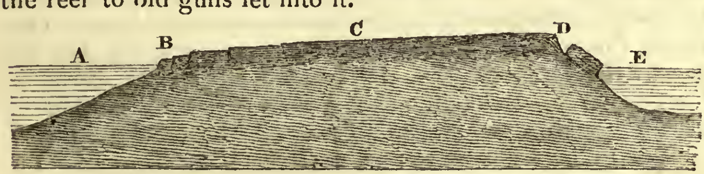
[THIRD SERIES.]

OCTOBER 1841.

XXXVII. *On a remarkable Bar of Sandstone off Pernambuco, on the Coast of Brazil.* By C. Darwin, *Esq., M.A., F.R. & G.S.**

IN entering the harbour of Pernambuco, a vessel passes close round the point of a long reef, which, viewed at high water when the waves break heavily over it, would naturally be thought to be of coral-formation, but when beheld at low water it might be mistaken for an artificial breakwater, erected by cyclopean workmen. At low tide it shows itself as a smooth level-topped ridge, from thirty to sixty yards in width, with even sides, and extending in a *perfectly* straight line, for several miles, parallel to the shore. Off the town it includes a shallow lagoon or channel about half a mile in width, which further south decreases to scarcely more than a hundred yards. Close within the northern point ships lie moored alongside the reef to old guns let into it.



Transverse section: vertical heights considerably exaggerated.

A. Level of the sea at low water.
B. Subsided masses, thickly coated with Serpulæ, &c.
C. Summit of the bar, which generally slopes a little seaward; but the slope in the woodcut has been unintentionally somewhat increased.
D. Subsided masses of bare sandstone.
E. Surface of the harbour or lagoon.

* Communicated by the Author.

The accompanying woodcut represents, at low water spring tides, a transverse section of the northern part of the bar, where a section of about seven feet in height is exhibited on the inner side. It consists of a hard pale-coloured sandstone, breaking with a very smooth fracture, and formed of siliceous grains, cemented by calcareous matter. Well-rounded quartz pebbles, from the size of a bean, rarely to that of an apple, are imbedded in it, together with a very few fragments of shells. Traces of stratification are obscure, but there was an included layer in one spot of stalactitic limestone, an eighth of an inch in thickness. In another place some false strata, dipping landwards at an angle of 45°, were capped by a horizontal mass. On each side of the ridge quadrangular fragments have subsided, as shown in the woodcut; and the whole mass is in some places fissured, apparently from the washing out of some soft underlying bed. One day, at low water, I walked a full mile along this singular, smooth, and narrow causeway, with water on both sides of me, and could see that for nearly a mile further its form remained unaltered. In Baron Roussin's beautiful chart of Pernambuco (*Le Pilote du Brésil*) it is represented as stretching on, in an absolutely straight line, for several leagues; how far its composition remains the same, I know not; but from the accounts I received from intelligent native pilots, it seems to be replaced on some parts of the coast by true coral reefs.

The upper surface, though on a large scale it must be called smooth, yet presents, from unequal disintegration, numerous small irregularities. The larger imbedded pebbles stand out supported on short pedestals of sandstone. There are, also, many sinuous cavities, two or three inches in width and depth, and from six inches to two feet in length. The upper edges of these furrows sometimes slightly overhang their sides; they end abruptly, but in a rounded form. One of the furrows occasionally branches into two arms, but generally they are nearly parallel to each other, and placed in lines transverse to the sandstone ridge. I know not how to account for their origin, without they be formed by the surf, as it daily breaks over the bar, washing to and fro pebbles in depressions, originally only slight. Opposed to this notion is the fact, that some of them were lined with numerous small living *Actineæ*. I have copied this passage, as I at the time wrote it, because furrows of a somewhat similar nature on the surface of rocks have lately received much attention, and are supposed invariably to indicate the former action of a waterfall, over the edge of a moving glacier.

The exterior part of the bar is coated with a thin layer of

calcareous matter; this, on the outer subsided masses, which can only be reached between the successively breaking waves at low water, is so thick, that I could seldom expose the sandstone with a heavy hammer. I procured, however, some fragments where the layer was between three and four inches in thickness; it consists chiefly of small *Serpulæ*, including some *Balani*, and a few very thin paper-like layers of a *Nullipora*. The surface alone is alive, and all within consists of the above organic bodies filled up with dirty white calcareous matter. The layer, though not hard, is tough, and from its rounded surface resists the breakers. Along the whole external margin of the bar, I only saw one very small point of sandstone which was exposed to the surf. In the Pacific and Indian Oceans the outer and upper margin of the coral reefs are protected, as will be described in a forthcoming work, by a very similar coating; but there it is almost exclusively formed of several species of *Nulliporæ*. Lieut. Nelson, in his excellent memoir on the Bermudas (Geol. Trans., vol. v. part 1. p. 117), has described reefs, formed, as he states, but I cannot avoid suspecting only coated, by similar masses of *Serpulæ*. I inquired from some old pilots, whether there was any tradition of change in the form and dimensions of this sandstone bar; but they were unanimous in answering me in the negative. It is astonishing to reflect, that although waves of turbid water, charged with sediment, are driven night and day, by the ceaseless trade-wind, against the abrupt edges of this natural breakwater, yet that it has lasted in its present perfect state for centuries, or more probably thousands of years. Seeing that the surface on the inner side does gradually wear away, as shown by the pebbles on the sandstone pedestals, this durability must be entirely owing to the protection afforded by the thin coating of *Serpulæ* and other organic beings: it is a fine example, how apparently inefficient, yet how effectual, are the means of preservation, like those of destruction, which nature employs.

I believe similar bars of rock occur in front of some of the other bays and rivers on the coast of Brazil: Baron Roussin states that at Porto Seguro there is a "*quay*" similar to that of Pernambuco. Spaces of several hundred miles in length on the shores of the Gulf of Mexico, the United States, and southern Brazil are formed by long narrow islands and spits of sand, including very extensive shallow lagoons, some of which are several leagues in width. The origin of these linear islets is rather obscure: Prof. Rogers (Report to British Association, vol. iii. p. 13.) gives some reasons for suspecting

that they have been formed by the upheaval of shoals, deposited where currents met. These phænomena, it is very probable, are connected in their origin with the same causes which have produced the remarkable bar of sandstone off Pernambuco. The town of Pernambuco stands on a low narrow islet and on a long spit of sand, in front of a very low shore, which is bounded in the distance by a semicircle of hills. By digging at low water near the town the sand is found consolidated into a sandstone, similar to that of the breakwater, but containing many more shells. If, then, the interior of a long sandy beach in one part, and in another the nucleus of a bar or spit extending in front of a bay became consolidated, a small change, probably of level, but perhaps simply in the direction of the currents, might give rise, by washing away the loose sand, to a structure like that in front of the town of Pernambuco, and along the coast southward of it; but without the protection afforded by the successive growth of organic beings, its duration would be short, if indeed it were not destroyed before being completely exhibited.

XXXVIII. *On the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis.* *By* James Prescott Joule, *Esq.**

1. THERE are few facts in science more interesting than those which establish a connexion between heat and electricity. Their value, indeed, cannot be estimated rightly, until we obtain a complete knowledge of the grand agents upon which they shed so much light. I have hoped, therefore, that the results of my careful investigation on the heat produced by voltaic action, are of sufficient interest to justify me in laying them before the Royal Society.

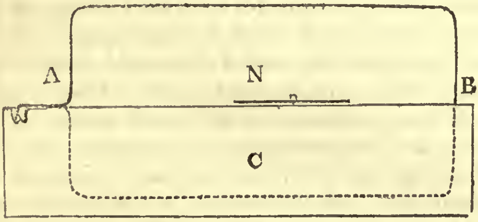
Chap. I.—*Heat evolved by Metallic Conductors.*

2. It is well known that the facility with which a metallic wire is heated by the voltaic current is in inverse proportion to its conducting power, and it is generally believed that this proportion is exact; nevertheless I wished to ascertain the fact for my own satisfaction, and especially as it was of the utmost importance to know whether resistance to conduction is the *sole* cause of the heating effects. The detail, therefore, of some experiments confirmatory of the law, in addition to those already recorded in the pages of science, will not, I hope, be deemed superfluous.

* Communicated by the Author.

3. It was absolutely essential to work with a *galvanometer*, the indications of which could be depended upon, as marking definite quantities of electricity. I bent a rod of copper into

Fig. 1.



the shape of a rectangle (A B, fig. 1.), twelve inches long, and six inches broad. This I secured in a vertical position by means of the block of wood C; N is the magnetic needle, $3\frac{3}{4}$ inches long, pointed at its extremities, and suspended upon a fine steel pivot over a graduated card placed a little before the centre of the instrument.

4. On account of the large relative size of the rectangular conductor of my galvanometer, the tangents of the deviations of the needle are very nearly proportional to the quantities of current electricity. The small correction which it is necessary to apply to the tangents, I obtained by means of the rigorous experimental process which I have some time ago described in the 'Annals of Electricity*'.

5. I have expressed my quantities of electricity on the basis of Faraday's great discovery of definite electrolysis, and I venture to suggest, that that quantity of current electricity which is able to electrolyze an atomic element expressed in grains in one hour of time, be called a *degree*. Now by a number of experiments I found that the needle of my galvanometer deviated 33°·5 of the graduated card, when a current was passing in sufficient quantity to decompose nine grains of water per hour; that deviation, therefore, indicates one *degree of current electricity* on the scale that I propose to be adopted. We shall see in the sequel some of the practical advantages which I have had by using this measure.

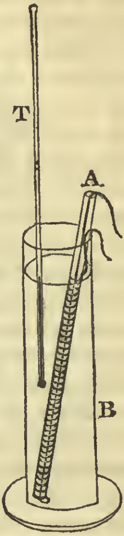
6. The thermometer which I used had its scale graduated on the glass stem. The divisions were wide, and accurate. In taking temperatures with it, I stir the liquid gently with a feather; and then, suspending the thermometer by the top of its stem, so as to cause it to assume a vertical position, I bring my eye to a level with the top of the mercury. In this

* Vol. iv. pp. 131–132, and 476.

way a little practice has enabled me to estimate temperatures to the tenth part of Fahrenheit's degree with certainty.

7. In order to ascertain the heating power of a given metallic wire, it was passed through a thin glass tube, and then closely coiled upon it. The extremities of the coil thus formed were then drawn asunder, so as to leave a small space between each convolution, and if this could not be well done, a piece of cotton thread was interposed. The apparatus thus prepared, when placed in a glass jar containing a given quantity of water, was ready for experiment. Fig. 2 will explain the dispositions: A is the coil of wire; B the glass jar partly filled with water; T represents the thermometer. When the voltaic electricity is transmitted through the wire, no appreciable quantity passes from it to take the shorter course through the water. No trace of such a current could be detected, either by the evolution of hydrogen, or the oxidation of metal.

Fig. 2.



8. Previous to each of the experiments, the necessary precaution was taken of bringing the water in the glass jar, and the air of the room to the same temperature. When this is accurately done, the results of the experiments bear the same proportions to one another as if no extraneous cooling agents, such as radiation, were present; for their effects in a given time are proportional to the difference of the temperatures of the cooling and cooled bodies; and hence, although towards the conclusion of some experiments this cooling effect is very considerable, the *absolute quantities* alone of heat are affected, not the *proportions* that are generated in the same time. [See the table of heats produced during half an hour and one hour, p. 264.]

9. Exp. 1.—I took two copper wires, each two yards long, one of them $\frac{1}{28}$th of an inch, the other $\frac{1}{30}$th of an inch thick, and arranged them in coils in the manner that I have described (7.). These were immersed in two glass jars, each of which contained nine ounces avoirdupois of water. A current of the mean quantity 1°·1 Q*, was then passed consecutively through both coils, and at the close of one hour I observed that the water in which the thin wire was immersed had gained 3°·4, whilst the thick wire had produced only 1°·3.

* I place Q at the end of my *degrees*, to distinguish them from those of the graduated card.

10. Now by direct experiment, I found that three feet of the thin wire could conduct exactly as well as eight feet of the thick wire; and hence it is evident that the resistances of two yards of each were in the ratio of 3·4 to 1·27, which approximates very closely to the ratio of the heating effects exhibited by the experiment.

11. Exp. 2.—I now substituted a piece of iron wire $\frac{1}{27}$th of an inch thick, and two yards long, for the thick copper wire used in Exp. 1, and placed each coil in half a pound of water. A current of 1°·25 Q was passed through both during one hour, when the augmentation of temperature caused by the iron was 6°, whilst that produced by the copper wire was 5°·5. In this case the resistances of the iron and copper wires were found to be in the ratio of 6 to 5·51.

12. Exp. 3.—A coil of copper wire was then compared with one of mercury, which was accomplished by inclosing the latter in a bent glass tube. In this way I had immersed, each in half a pound of water, 11¼ feet of copper wire $\frac{1}{30}$th of an inch thick, and 22¾ inches of mercury 0·065 of an inch in diameter. At the close of one hour, during which the same current of electricity was passed through both, the former had caused a rise of temperature of 4°·4, the latter of 2°·9. The resistances were found by a careful experiment to be in the ratio of 4·4 to 3.

13. Other trials were made with results of precisely the same character; they all conspire to confirm the fact, that *when a given quantity of voltaic electricity is passed through a metallic conductor for a given length of time, the quantity of heat evolved by it is always proportional to the resistance* which it presents, whatever may be the length, thickness, shape, or kind of that metallic conductor.*

14. On considering the above law, I thought that the effect produced by the increase of the intensity of the electric current would be as the square of that element, for it is evident that in that case the resistance would be augmented in a double ratio, arising from the increase of the *quantity* of electricity passed in a given time, and also from the increase of the *velocity* of the same. We shall immediately see that this view is actually sustained by experiment.

15. I took the coil of copper wire used in Exp. 3, and have found the different quantities of heat gained by half a pound of water in which it was immersed, by the passage of electri-

* Mr. Harris, and others, have proved this law very satisfactorily, using common electricity.

cities of different degrees of tension. My results are arranged in the following table:—

Mean Deviations of the Needle of the Galvanometer.	Quantities of Current Electricity expressed in Degrees (5.)	Quantities of Heat produced in half an hour by the Intensities in Column 2.	Ratio of the Squares of the Intensities in Column 2.	Quantities of Heat produced in one hour by the Intensities in Column 2.	Ratio of the Squares of the Intensities in Column 2.
16°	0°·43 Q	......°	......	1°·2	1
31½	0·92 Q	3	2·9	4·7	4·55
55	2·35 Q	19·4	18·8		
57⅔	2·61 Q	23	23·2		
58½	2·73 Q	25	25·4	39·6	40·

16. The differences between the numbers in columns three and five, and those in columns four and six, are very inconsiderable, taking into account the nature of the experiments, and are principally owing to the difficulty which exists in keeping the air of the room in the same state of quiet, of hygrometry, &c. during the different days on which the experiments were made. They are much less when a larger quantity of water is used, so as to reduce the cooling effects (28.).

17. We see, therefore, that *when a current of voltaic electricity is propagated along a metallic conductor, the heat evolved in a given time is proportional to the resistance of the conductor multiplied by the square* of the electric intensity.*

18. The above law is of great importance. It teaches us the right use of those instruments which are intended to measure electric currents by the quantities of heat which they evolve. If such instruments be employed (though in their present state they are far inferior in point of accuracy to many other forms of the galvanometer), it is obvious that the *square roots* of their indications are alone proportional to the intensities which they are intended to measure.

19. By another important application of the law, we are now enabled to compare the frictional† and voltaic electri-

* The experiments of De la Rive show that the calorific effect of the voltaic current increases in a much greater proportion than the simple ratio of the intensities.—*Ann. de Chimie*, 1836, part i. p. 193. See also Peltier's results, *Ann. de Chimie*, 1836, part ii. p. 249.

† The experiments of Brooke, Cuthbertson and others, prove that the quantity of wire melted by common electricity is as the square of the battery's charge. Harris, however, arrived at the conclusion, that the heating power of electricity is *simply* as the charge, Phil. Trans., 1834,

cities, in such a manner as to determine their elements by the quantity of heat which they evolve in passing along a given conductor; for if a certain quantity of voltaic electricity produce a certain degree of heat by passing along a given conductor, and if the same quantity of heat be generated by the discharge of a certain electrical battery along the same conductor, the product of the quantity and velocity of transfer of the *voltaic* electricity will be equal to the product of the quantity and velocity of the *frictional* electricity, or $Q\,V = q\,V$, whence $\frac{Q}{q} = \frac{v}{V}$.

CHAP. II.—*Heat evolved during Electrolysis.*

20. Under the above head, I shall now examine the heat produced in the cells of the battery, and when electrolytes are experiencing the action of the voltaic current. It has been my desire to render these experiments strictly comparable, both with themselves and with those of other philosophers. I have therefore taken care to apply the corrections which either specific heat, or other disturbing causes might require, and have by these means been able to express, in every case, the *total* amount of evolved heat.

21. The first of these corrections, which I call Cor. A, arises from the difference between the mean temperature of the liquid used in an experiment, and that of the surrounding atmosphere. Its amount is determined by ascertaining the rapidity with which the temperature of the liquid is reduced at the end of each experiment.

22. The second correction (Cor. B) is for the specific heat of the liquids, and the vessels which contain them; and when the necessary data could not be found in the tables of specific heat, I have supplied them from my own experiments. The *vessels* were white earthenware jars, 4½ inches deep, and 4¼ inches in diameter: their caloric was one-twelfth of that contained by two pounds of water, *to which capacity I have reduced all my subsequent results.*

23. As resistance to conduction is the sole cause of the heat produced in the connecting wire of the voltaic battery, it was natural to expect that it would act an important part in this second class of phænomena also. It was important, therefore, to begin by determining the amount of heat evolved by that quantity of conducting metal which I found it convenient to adopt as a *standard of resistance.*

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p. 225. Of course the remark in the text is made on the presumption, that when the proper limitations are observed, the calorific effect of electricity is as the square of the charge of any given battery.

24. Ten feet of copper wire, 0·024 of an inch thick, were formed into a coil in the manner described in (7.); its resistance to conduction was called *unity*. Three experiments were made in order to ascertain its heating power.

25. 1st. A jar was filled with two pounds of water, and a current which produced a mean deviation of the needle of the galvanometer (3.) equal to $57\frac{1}{4}^\circ = 2^\circ{\cdot}54$ Q of current electricity, was urged through the coil for twenty-seven minutes, by means of a zinc-iron* battery of ten pairs. The heat thus acquired by the water, after Cor. A, and that part of Cor. B which relates to the caloric of the jar, had been applied, was $6^\circ{\cdot}22$.

26. 2nd. The battery was now charged with a weaker solution of sulphuric acid. In this case it passed the mean current $2^\circ{\cdot}085$ Q during forty-five minutes. The heat thus produced, when corrected, was $7^\circ{\cdot}04$.

27. 3rd, A battery of five pairs (three of which had platinized silver; one silver, and one copper, for their negative plates,) passed the mean current $1^\circ{\cdot}88$ Q during one hour, in which time $7^\circ{\cdot}47$ were generated.

28. When the first two experiments are reduced, in order to compare them with the third, we have, in accordance with the principles laid down in (17.), $\frac{(1{\cdot}88)^2}{(2{\cdot}54)^2} \times \frac{60'}{27'} \times 6^\circ{\cdot}22 = 7^\circ{\cdot}57$, and $\frac{(1{\cdot}88)^2}{(2{\cdot}085)^2} \times \frac{60'}{45'} \times 7^\circ{\cdot}04 = 7^\circ{\cdot}63$. Thus we have $\frac{7^\circ{\cdot}57 + 7^\circ{\cdot}63 + 7^\circ{\cdot}47}{3} = 7^\circ{\cdot}56$, the mean and total quantity of heat produced per hour by the passage of $1^\circ{\cdot}88$ Q of current electricity, against the *unit* of resistance.

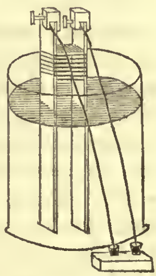
29. Before I proceed to give an account of some experiments on heat evolved in the cells of voltaic pairs, it is important to observe that every kind of action not essentially electrolytic must be eliminated. For instance, the dissolution of metallic oxides in acid menstrua, which has been proved by Dr. Faraday to be no cause of the current, is the occasion of a very considerable quantity of heat, which, if not accounted for in the experiments, would altogether disturb the results. I have taken the oxide of zinc, prepared either by igniting the nitrate, or by burning the metal, and have repeatedly dissolved it in sulphuric acid of various specific gravities; and on taking the mean of many experiments, none of which dif-

* Whenever an iron battery was used, it was of course placed at a distance from the galvanometer sufficiently great to render its action on the needle altogether inappreciable.

fered materially from the rest, I have found that the total corrected heat produced by the dissolution of 100 grains of the oxide of zinc in sulphuric acid, is able to raise two pounds of water 3°·44.

30. Exp. 1.—I constructed a single voltaic pair, consisting of thin plates of amalgamated zinc and platinized silver (Mr. Smee's arrangement): the plates were two inches broad, and were kept one inch asunder by means of a piece of wood, to the opposite sides of which they were bound with string: to the top of each plate, a thick copper wire formed a good metallic connexion, by means of a brass clamp. The voltaic pair, thus prepared, was immersed in two pounds of sulphuric acid, sp. gr. 1137, contained by one of the earthenware jars (22.). The arrangement is represented by fig. 3.

Fig. 3.



31. When the circuit was completed so as to present to the current the total metallic resistance 0·06, the galvanometer stood at $49\frac{1}{2}° = 1°·84$ Q; and at $17\frac{1}{2}° = 0°·453$ Q, when the total metallic resistance was increased to 1·16 by the addition to the circuit of ten feet of thin copper wire. Hence, according to the principles laid down by Ohm, $\frac{1·84}{r+1·16} = \frac{0·453}{r+0·06}$; from which r, the resistance of the voltaic pair, = 0·299. Immediately after this trial, the temperature of the liquid being exactly 49°, and that of the air 50°·2, the circuit was completed for one hour, during which the needle first advanced a little from 50°, and then declined to 46°, the average* deviation was 48° 44′ = 1°·8 Q. The temperature of the liquid was then 53°·7, indicating a rise of 4°·7. Another trial now ave $\frac{1·59}{r'+1·16} = \frac{0·382}{r'+0·06}$; whence r', the resistance of the pair at the close of the experiment, = 0·288: the mean resistance of the pair was therefore 0·293.

32. Now in order to obtain the total amount of heat evolved by the pair, reduced to the capacity of two pounds of water, we have 4°·7 + 0°·4 (on account of Cor. A (21.)) and − 0°·5 (on account of Cor. B (22.)) = 4°·6. The correction due to the dissolution of oxide of zinc is found by multiplying its

* During each experiment the deflections of the needle were noted at intervals of five minutes, or less. From thence I deduce my averages.

quantity by $\frac{3^\circ{\cdot}44}{100}$ (see (29.)); the quantity of the oxide being obtained by multiplying the equivalent of oxide of zinc by the mean quantity of current electricity. We have then $4\ 0{\cdot}3 \times 1{\cdot}8 \times \frac{3^\circ{\cdot}44}{100} = 2^\circ{\cdot}5$: this, when subtracted from $4^\circ{\cdot}6$, leaves $2^\circ{\cdot}1$, the *correct voltaic heat.*

33. Assuming in this case, as well as in that of a metallic conductor, that the heat evolved is proportional to the resistance multiplied by the square of the electric intensity, we have, from the data in (28.) and (31.), $\frac{(1{\cdot}8)^2}{(1{\cdot}88)^2} \times 0{\cdot}293 \times 7^\circ{\cdot}56 = 2^\circ{\cdot}03$, which is very near $2^\circ{\cdot}1$, the heat deduced from experiment.

34. Exp. 2.—I now constructed another pair, consisting of plates precisely similar to those used in Exp. 1, but half an inch only asunder: it was also immersed in two pounds of sulphuric acid, sp. gr. 1137. The circuit was closed for one hour, during which the needle of the galvanometer advanced gradually from $47\frac{1}{2}^\circ$ to $50\frac{1}{3}^\circ$, the mean deviation being $49^\circ\ 35' = 1^\circ{\cdot}84$ Q. The liquid had then gained $4^\circ{\cdot}8$: this, $+0^\circ{\cdot}1$ (for Cor. A) and $-0^\circ{\cdot}5$ (for Cor. B), $= 4^\circ{\cdot}4$. The heat due to the dissolution of oxide of zinc is in this case $4\ 0{\cdot}3 \times 1{\cdot}84 \times \frac{3^\circ{\cdot}44}{100} = 2^\circ{\cdot}55$, which, when subtracted from $4^\circ{\cdot}4$, leaves the correct voltaic heat $1^\circ{\cdot}85$.

35. The resistance of the pair was ascertained in this, as in every other instance, at the beginning and at the end of the experiment. The equations thus obtained were $\frac{1{\cdot}714}{r + 1{\cdot}16} = \frac{0{\cdot}432}{r + 0{\cdot}06}$, and $\frac{1{\cdot}91}{r' + 1{\cdot}16} = \frac{0{\cdot}446}{r' + 0{\cdot}06}$; whence $r = 0{\cdot}311$, and $r' = 0{\cdot}275$: the mean resistance was therefore $0{\cdot}293$. Now, calculating as before (33.), on the basis of the heat produced by the passage of electricity against the standard of resistance, we have $\frac{(1{\cdot}84)^2}{(1{\cdot}88)^2} \times 0{\cdot}293 \times 7^\circ{\cdot}56 = 2^\circ{\cdot}12$.

36. Exp. 3.—I formed another pair on Mr. Smee's plan; it was similar to the last, with the exception that the plates were only one inch broad. When the circuit was closed, a current of the mean intensity $1^\circ{\cdot}46$ Q passed through the apparatus during one half hour. The heat thereby produced,

when corrected, and reduced on account of the dissolution of oxide of zinc, was $0^{\circ}{\cdot}84$.

37. In this instance the mean resistance was 0·32 ; whence, by a calculation precisely similar to those given under Exps. 1 and 2, we have the theoretical amount of heat $= 0^{\circ}{\cdot}74$.

38. The three instances above given, are specimens taken from a number of experiments with the platinized silver pairs. The mean of the eight unexceptionable experiments which I have made with them, gives $2^{\circ}{\cdot}08$ of actual, and $2^{\circ}{\cdot}13$ of theoretical heat, and not one of the individual experiments presented a greater difference between real and calculated heat, than Exp. 2.

39. Exp. 4.—A plate of copper, four inches broad, was bent about a plate of amalgamated zinc three inches and a half broad, so as to form a pair of Wollaston's double battery. It was placed in a jar containing two pounds of dilute sulphuric acid. In this instance, the total voltaic heat that was generated was $1^{\circ}{\cdot}2$, the calculated result being $1^{\circ}{\cdot}0$ only. Repeated experiments with the copper pairs gave similar results, the real heat being invariably somewhat superior to that which the doctrine of resistances would demand. The cause of this I have found in a slight local action, which it is almost impossible to avoid in the common copper battery.

40. Exp. 5.—I now constructed a single pair on Mr. Grove's plan. The platinum, two inches broad, was immersed in an ounce and a half of strong nitric acid contained by a 4-inch pipe-clay cell; the amalgamated zinc plate, also two inches broad, was immersed (at the distance of an inch and a half from the platinum) in thirty ounces of sulphuric acid, sp. gr. 1156. The whole was contained by one of the jars (22.).

41. A trial, made first as usual, in order to ascertain the resistance of the pair, gave $\frac{4{\cdot}4}{r+2{\cdot}26} = \frac{0{\cdot}816}{r+0{\cdot}06}$, whence $r = 0{\cdot}441$. As soon as the slight heat acquired during the above trial was equably diffused through the apparatus, the thermometer placed in the dilute sulphuric acid stood at $51^{\circ}{\cdot}95$, the temperature of the air being $52^{\circ}{\cdot}4$. The circuit was then immediately closed for ten minutes, during which time the needle of the galvanometer advanced steadily from $68^{\circ}\ 40'$ to $71^{\circ}\ 20'$; the mean deviation being $70^{\circ}\ 9' = 4^{\circ}{\cdot}77$ Q. As soon as the heat thus generated was equably diffused*,

* By gently stirring the dilute sulphuric acid with a feather, so as to bring every part in successive contact with the porous cell during two minutes.

the thermometer immersed in the dilute sulphuric acid stood at $56^\circ{\cdot}7$, indicating a rise of $4^\circ{\cdot}75$. Another trial now gave $\frac{5{\cdot}14}{r'+2{\cdot}26} = \frac{0{\cdot}91}{r'+0{\cdot}06}$, whence $r' = 0{\cdot}413$. The mean resistance of the pair was therefore 0·427.

42. $4^\circ{\cdot}75 + 0^\circ{\cdot}1$ (for Cor. A), and $- 0^\circ{\cdot}4$ (for Cor. B, which in this case includes the capacity for heat of the porous cell) $= 4^\circ{\cdot}45$. The heat generated by the dissolution of oxide of zinc was in this case $40{\cdot}3 \times 4{\cdot}77 \times \frac{3^\circ{\cdot}44}{100} \times \frac{10}{60'}$ $= 1^\circ{\cdot}1$, which, subtracted from $4^\circ{\cdot}45$, leaves the correct voltaic heat $3^\circ{\cdot}35$.

43. The *theoretical* result is $\frac{(4{\cdot}77)^2}{(1{\cdot}88)^2} \times 0{\cdot}427 \times 7^\circ{\cdot}56 \times \frac{10'}{60'}$ $= 3^\circ{\cdot}46$.

44. Exp. 6 was made with a pair in every respect similar to the last: the circuit, however, was completed by means of a thin copper wire, in order to reduce the intensity of the current. At the end of one hour, during which the needle of the galvanometer advanced gradually from 41° to 42°, the correct voltaic heat that was generated was $1^\circ{\cdot}7$. The *theoretical* result was $1^\circ{\cdot}82$.

45. I was desirous of knowing how far the same principles would apply to the heat generated in Prof. Daniell's constant battery. But in this battery considerable *cold* is produced, in consequence of the separation of oxide of copper from the sulphuric acid to which it is combined. This is altogether a secondary effect, and should be eliminated as decidedly as the *heat* produced by the dissolution of oxide of zinc. I have not yet been able to obtain accurate data for the correction thus needed, and shall therefore content myself with remarking, that my results with Mr. Daniell's arrangements are, as far as they go, quite consistent with the theory of resistances.

46. Experiments, such as I have related, were varied in many ways; and sometimes a number of pairs were arranged so as to form a battery. Still the results were similar, and established the fact, that *the heat which is generated in a given time in any pair, by true voltaic action, is proportional to the resistance to conduction of that pair, multiplied by the square of the intensity of the current.*

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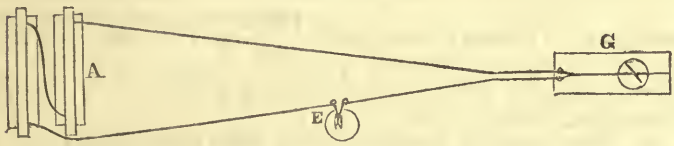
47. I now made some experiments on the heat consequent to the passage of voltaic electricity through electrolytes.

48. Exp. 7.—Two pieces of platinum foil, each of which was an inch long, and a quarter of an inch broad, were her-

metically sealed into the ends of two pieces of glass tubing: within these tubes, pieces of copper wire were metallically connected with the platinum; these, when the apparatus was in action, terminated in mercury cups. The tubes thus prepared were bound together by thread, so as to keep the pieces of platinum foil at the constant distance of half an inch asunder. This apparatus was immersed in two pounds of dilute sulphuric acid, sp. gr. 1154, contained in one of the jars (22.).

49. A battery of twenty-four inch, double iron-zinc plates, was then placed, with its divided troughs (which were charged with a pretty strong solution of sulphuric acid), at a distance from the galvanometer sufficiently great to obviate any disturbing effect on the needle. To the electrodes of this battery thick copper wires were secured, so that by means of one of them connexion could be made to the galvanometer, and by means of the other, to the decomposing cell. In fig. 4, A

Fig. 4.



represents the battery, G the galvanometer, and E the decomposing apparatus (48.).

50. In order to ascertain the resistances of the battery or of the cell, I provided several coils* of silked copper wire, the resistances of which had been determined by careful experiments. When these were traversed by the current, they were placed in such a position as to prevent any action on the needle, and at the same time they were kept under water, in order to prevent them from becoming hot, which would have had the effect of increasing their resistances.

51. When everything was duly prepared, the battery was placed in its troughs, and the current from it was urged through the galvanometer and each of three of the coils, which were placed in succession at E (the decomposing apparatus having been removed). The resistances of these coils were 4·4, 5·5, and 7·7, and the currents which they allowed to pass were 1°·88 Q, 1°·65 Q, and 1°·29 Q.

52. The decomposing apparatus was now replaced, and the proper connexions being made, electrolytic decomposition

* Two of these coils had been previously employed (31. 41.), &c. in ascertaining the resistances of the voltaic pairs: the resistance 0·06 was that of the galvanometer and connecting wires.

was allowed to proceed during twenty minutes, in which time the needle of the galvanometer gradually declined from 55° to 48½°, the mean current being 1°·9 Q. The temperature of the liquid had now advanced from 46°·6 to 53°·95, indicating an increase of 7°·35. The temperature of the surrounding atmosphere was 46°·4.

53. The decomposing cell was now removed again, and the several coils, of which the resistances were, as before, 4·4, 5·5, and 7·7, were successively put in its place. The battery now urged through them, 1°·73 Q, 1°·48 Q, and 1°·22 Q.

54. In this case 7°·35 + 0°·55 (for Cor. A) and − 0°·64 (for Cor. B) = 7°·26, the heat which was generated in the decomposing jar.

55. The mean intensity of the current when passing through the coil of which the resistance was 4·4, was $\frac{1°{\cdot}88 + 1°{\cdot}73}{2}$ = 1°·805 Q, but 1°·9 Q when it passed through the decomposing cell. Hence $(4{\cdot}4 + 3{\cdot}15^*)\frac{1{\cdot}805}{1{\cdot}9} = 7{\cdot}17$, this, −3·15*, leaves 4·02, the amount of *obstruction* presented by the decomposing cell.

56. Now we must remember, that when the electric current was passing through the coils, it was urged by the whole intensity of the battery; but that in the case of the decomposing cell, a part of the intensity of the zinc-iron battery, equal (as I have found by experiment) to 3⅓ pairs, or to one sixth part of the whole, is occupied solely in overcoming the *resistance to electrolyzation* † of water in the decomposing cell. In order therefore to deduce the true *resistance to conduction*, we must subtract $\frac{4{\cdot}02 + 3{\cdot}15}{6}$ from the *obstruction* 4·02; and thus we have 2·83, the true *resistance to conduction* of the decomposing cell.

57. The latter part of this process is difficult to express clearly, I have therefore drawn a figure to illustrate it. Suppose that in fig. 5, 6 represents the intensity of the battery; the line R 3·15, the resistance of the battery and the connecting wires; and the remainder of the line A B, or 4·02 W,

* From (51. 53.) we have the equations $\frac{1{\cdot}88}{R+7{\cdot}7} = \frac{1{\cdot}29}{R+4{\cdot}4}$ and $\frac{1{\cdot}73}{R'+7{\cdot}7} = \frac{1{\cdot}22}{R'+4{\cdot}4}$, whence R = 2·81 and R′ = 3·49 : the mean resistance of the battery and connecting wires was therefore 3·15.

† Faraday's Experimental Researches, (1007).

the resistance of wire. I have shown, (55.) that the cur-

Fig. 6.

Fig. 5.

rent 1°·9 Q would pass against the resistance A B. But we know that 1°·9 Q was also passed when the cell and the battery formed the sole opposition (52.), and that on account of the resistance to electrolyzation, the virtual battery intensity was then one-sixth less, and hence that only five-sixths of the resistance represented by A B could have been opposed in this case, in order to the passage of the same current. Draw, therefore, another line, C D, one-sixth less than A B, and it will represent this resistance; from which, on subtracting R 3·15, we have r 2·83, the true resistance to conduction of the decomposing apparatus.

58. From (28.), and the data above given, we have $\frac{(1·9)^2}{(1·88)^2} \times 2·83 \times 7°·56 \times \frac{20'}{60'} = 7°·29$, the *theoretical* result.

59. I made three other experiments with the same electrodes, and with the same battery. The results of these with those of the experiment just given at length, are as follows:—

	Experimental.		Theoretical.
Exp. 7.	7°26	....	7°29
Exp. 8.	8·12	....	8·32
Exp. 9.	10·2	....	10·2
Exp. 10.	9·64	....	9·75 (Refitted battery.)
Mean	8·8	....	8·89

60. Exp. 11.—The mean current 0°·846 Q from a battery of ten zinc-iron pairs, was, by means of the same electrodes, sent through two pounds of dilute sulphuric acid for half an hour, during which the correct heat that was generated was 3°·09.

61. In order to find the true resistance to conduction of the decomposing cell, it was necessary to remember that in this instance one-third of the intensity of the ten pairs was expended in overcoming the resistance to electrolyzation of the water. With this exception the calculations were made precisely as before, and gave 3·76, the resistance of the cell; whence we have the theoretical heat 2°·88.

62. I now dismissed the narrow electrodes, and substituted for them two pieces of platinum foil, dipping to the bottom of the liquid; they were one inch apart, and each presented to the dilute sulphuric acid a surface of seven square inches. In this case I used twenty pairs of zinc-iron plates arranged in a series of ten.

63. The mean of six experiments with this apparatus gave $4°·42$ of real, and $4°·13$ of theoretical heat. I have no doubt that the difference is principally occasioned by the formation of the deutoxide of hydrogen, which is known to occur to a considerable extent when oxygen is evolved from an extended surface. Of this we have another instance in the following experiment.

64. Exp. 12.—Using the same electrodes, and a battery of ten zinc-iron pairs, I now passed a current of the mean intensity $1°·08$ Q through two pounds of dilute nitric acid, sp. gr. 1047, for half an hour. The heat that was thus generated, when properly corrected, was 3°.

65. This experiment was, as the others, conducted in the manner described at length under Exp. 7. Water chiefly* was decomposed; and I ascertained, experimentally, that about $\frac{1}{3·5}$ of the intensity of the battery was expended in overcoming resistance to electrolysis. Thus I had $3·52 - \frac{3·52 + 1·68}{3·5} = 2·03$, the resistance to conduction; and hence $\frac{(1·08)^2}{(1·88)^2} \times 2·03 \times 7°·56 \times \frac{30'}{60'} = 2°·53$, the theoretical heat.

66. Exp. 13.—Two plates of copper, each of which was two inches broad, were secured at the distance of one inch apart, and immersed in two pounds of a saturated solution of sulphate of copper. Through this apparatus, a battery of ten zinc-iron pairs passed the mean current 1° Q during half an hour. The heat thus produced, when properly corrected, was $5°·8$.

67. In this case there was no *resistance to electrolysis*, and the action may be regarded simply as a transfer of copper from the positive to the negative electrode. All the *obstruction*, therefore, that was presented to the current, was *resistance to conduction*. Its mean was 5·5, whence we have $\frac{1}{(1·88)^2} \times 5·5 \times 7°·56 \times \frac{30'}{60'} = 5°·88$, the theoretical heat.

* See Faraday on the Electrolysis of Nitric Acid, 'Experimental Researches,' (752.).

68. We have thus arrived at the general conclusion, that *the heat which is evolved by the proper action of any voltaic current is proportional to the square of the intensity of that current, multiplied by the resistance to conduction which it experiences.* From this law the following conclusions are directly deduced:—

69. 1st. That *if the electrodes of a galvanic pair of given intensity be connected by any simply conducting body, the total voltaic heat generated by the entire circuit* (*provided always that no local action occurs in the pair*) *will, whatever may be the resistance to conduction, be proportional to the number of atoms* (*whether of water or of zinc*) *concerned in generating the current.* For if the resistance to conduction be diminished, the quantity of current will be increased in the same ratio, and hence, according to the law (68.), the quantity of heat which would thus be generated in a given time will be also proportionally increased; whilst of course the number of atoms which would be electrolyzed in the pair will be increased in the same proportion.

70. 2nd. That *the total voltaic heat which is produced by any pair, is directly proportional to its intensity, and the number of atoms which are electrolyzed in it.* For the quantity of current is proportional to the intensity of the pair, and consequently the quantity of heat evolved in a given time is proportional to the square of the intensity of the pair, but the number of atoms electrolyzed is proportional, in the same time, to the simple ratio only of the current, or of the intensity of the pair.

71. And 3rd. That *when any voltaic arrangement, whether simple or compound, passes a current of electricity through any substance, whether an electrolyte or not, the total voltaic heat which is generated in any time, is proportional to the number of atoms which are electrolyzed in each cell of the circuit, multiplied by the virtual* intensity of the battery.*

72. Berzelius thinks that the light and heat produced by combustion are occasioned by the discharge of electricity between the combustible and the oxygen with which it is in the act of combination; and I am of opinion that the heat arising from this, and some other chemical processes, is the consequence of resistance to electric conduction. My experiments on the heat produced by the combustion of zinc turnings in oxygen, (which, when sufficiently complete, I shall make public) strongly confirm this view; and the quantity of heat which Crawford produced by exploding a mixture of hydrogen and

* If a decomposing cell be in the circuit, the *virtual* intensity of the battery is reduced in proportion to its resistance to electrolyzation.

oxygen may be considered almost decisive of the question. In his unexceptionable experiments, one grain of hydrogen produced heat sufficient to raise one pound of water $9^\circ\cdot6$. Now we know from Exp. 5, that the heat generated in one of Mr. Grove's pairs by the electrolysis of $\frac{4\cdot77 \times 32\cdot3}{6} = 25\cdot7$ grains of zinc, is theoretically $3^\circ\cdot46$; and the heat which must in the same time have been generated by the *metallic* part of the circuit, which presented the resistance 0·06, is $\frac{0\cdot06}{0\cdot427} \times 3^\circ\cdot46 = 0^\circ\cdot48$: the total voltaic heat was therefore $3^\circ\cdot94$. Hence the total heat which would have been evolved by the electrolysis of an equivalent, or 32·3 grains of zinc, is $\frac{32\cdot3}{25\cdot7} \times 3^\circ\cdot94 = 4^\circ\cdot95$; which, when reduced to the capacity of one pound of water, is $9^\circ\cdot9$. But from the table of the intensities of voltaic arrangements (74.), the intensity of Mr. Grove's pair, compared with the affinity of hydrogen for oxygen, is $\frac{1}{0\cdot93}$; whence, from (70.), we have $9^\circ\cdot9 \times 0\cdot93 = 9^\circ\cdot2$, the heat which should be generated by the combustion of one grain of hydrogen, according to the doctrine of resistances: the result of Crawford is only $0^\circ\cdot4$ more.

73. I am aware that there are some anomalous conditions of the current which seem to militate against the general law (68.), particularly when in the hands of Peltier it actually produces *cold**. I have little doubt, however, that the explanations of these will be ultimately found in actions of a secondary character.

Note on Voltaic Batteries.

74. In the foregoing investigation I have had occasion to work very extensively with different voltaic arrangements, and have repeatedly ascertained their relative intensities by the mathematical theory of Ohm. It will not, therefore, I hope, be deemed out of place to subjoin a table, in which the intensities of the batteries which are most generally used, are inversely as the number of pairs which would be just requisite in order to overcome the resistance of water to electrolyzation.

* If antimony and bismuth be soldered together, cold will be produced at the point of junction by the passage of the current from the bismuth to the antimony. Peltier, *Annales de Chimie*, vol. lvi. p. 371. In his paper, however, a misprint has inverted the direction of the current.

Mr. Grove's.	Platinum Nitric acid	Amalgam. zinc Dilute sulp. acid	$\frac{1}{0{\cdot}93}$	Constant Intensities.
Prof. Daniell's.	Copper Sulphate of oxide of copper	Amalgam. zinc Dilute sulphuric acid	$\frac{1}{1{\cdot}54}$	
Mr. Sturgeon's.	Iron. Amalgamated zinc Dilute sulphuric acid		$\frac{1}{3{\cdot}33}$	
Mr. Smee's.	Platinized silver. Amalg. zinc Dilute sulphuric acid		$\frac{1}{3{\cdot}58}$	
	Copper. Amalgamated zinc Dilute sulphuric acid		$\frac{1}{5{\cdot}40}$	

75. Without entering particularly into the respective merits of these arrangements, I may observe that each of the first four may be used advantageously, according to the circumstances in which the experimenter is placed, or the particular experiments which he wishes to execute. The zinc-iron battery is somewhat inconvenient on account of local action on the iron; but then it presents great mechanical facilities in its construction. Mr. Smee's and Mr. Grove's are also very good arrangements; but the battery of Daniell is the best instrument for general use, and is, moreover, unquestionably the most œconomical.

Broom Hill, Pendlebury near Manchester,
March 25, 1841.

P.S. In the above table of galvanic intensities, that of zinc-iron immersed in dilute sulphuric acid is somewhat overstated. Recent experiments convince me that when the iron is in its best condition it possesses the same powers as the platinized silver. I attributed the iron battery to Mr. Sturgeon, who constructed one of these excellent instruments early in 1839*. It consisted of twelve cast-iron tubes, furnished with strips of amalgamated zinc. But I find that the experiments of this gentleman were not published as early as those of Mr. Roberts. Prof. Daniell (Phil. Trans. 1836, p. 114) observed that iron is sometimes more efficient than platinum in voltaic association with amalgamated zinc.

August 11, 1841. J. P. J.

[* A paper by Dr. A. Fyfe, on the employment of iron in the construction of voltaic batteries, appeared in Phil. Mag. for August 1837: S. 3, vol. xi. p. 145.—Edit.]

XXXIX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 195.]

New Method of determining Nitrogen.

IN a letter addressed by Berzelius to Prof. Erdmann, and published in the *Journ. f. Prakt. Chem.* for June 15th, 1841, we find the following important notice:—"On preparing the interesting memoir of Dumas on the decomposition of organic substances, by the action of the hydrate of potash, for my Annual Report, it occurred to me that nitrogenous bodies should, when so treated, give off the whole amount of their nitrogen as ammonia, which may be received in muriatic acid just in the same manner as carbonic acid in a solution of potash, and weighed as ammonio-chloride of platinum. I am following up this idea with M. Plantamour. To guard against the formation of cyanogen, we have commenced with compounds of cyanogen, which give ammonia just as well as sal-ammoniac and lime. If, indeed, all should succeed according to the present appearances, what a valuable and easy control in determinations of nitrogen we shall thus obtain! We make the experiments in the same way as the usual organic analyses, and pass the vapours over a strongly-heated mixture of the hydrate of potash and hydrate of lime, to decompose such bodies as, for instance, anil $(C^{12} H^{8} + \underline{N} \, \underline{H}^{3})$."

Nitrurets.

On exposing the white precipitate to a gradually increasing heat, a vast quantity of ammonia first passes over without any trace of nitrogen gas, after which perchloride of mercury-ammonia, and ammonia are evolved; and a red body remains in the retort, which decomposes at a temperature above 360° into protochloride of mercury, mercury and nitrogen. This red body is obtained purest when the heating is continued in a metallic bath until some protochloride of mercury has formed. It consists of crystalline scales, and has quite the appearance of the crystalline peroxide of mercury. It is insoluble in water, not affected by aqueous alkalies, even at the boiling point of the liquid. The compound may be boiled with dilute and concentrated nitric acid, and with rather concentrated sulphuric acid, without being decomposed or dissolved, which takes place, however, when it is boiled with concentrated sulphuric acid, or with hydrochloric acid: no gas is evolved. Heated beyond the boiling point of mercury, nitrogen escapes; mercury and protochloride of mercury are sublimed. Several experiments were made to determine these three bodies; ac-

cording to which the compound consists of perchloride of mercury with nitruret of mercury, $2\ Hg\,\underline{Cl} + Hg^3\,\underline{N}$. Prof. Mitscherlich, by whom the above researches have been made, did not succeed in isolating the nitruret of mercury. This has been effected by Plantamour. It explodes violently, equalling, if not surpassing, in this respect the iodide of nitrogen, but is by far less sensitive. It forms a dark brown powder; decomposed with oxide of copper after the manner of an organic analysis, it gives no water, but only nitrogen and mercury; the true discoverer of nitrurets is, however, Schrötter, to whose experiments we shall presently return. Mitscherlich concludes, from the composition of the chloronitruret of mercury (*Quecksilberstickstoffchlorid*), that the white precipitate does not consist of one atom of the perchloride of mercury and one atom of amidide of mercury, $Hg\,\underline{Cl} + Hg\,\underline{N}\,\underline{H}^2$, but of three atoms of both substances, $3\ Hg\,\underline{Cl} + 3\ Hg\,\underline{N}\,\underline{H}^2$; as, on heating, two atoms of ammonia and one of perchloride of mercury are separated*. The perbromide of mercury acts in the same manner as the perchloride towards ammonia, giving off, on being heated, bromide of mercury-ammonia and ammonia, and bromo-nitruret of mercury remains behind. The mercury behaves in these compounds in the same way as potassium to ammonia; the olive-coloured body which is obtained by the action of potassium on dry gaseous ammonia is amidide of potassium, $(3\,K\,.\,N\,H^2)$; and the graphite-like substance, which is formed by exposing the amidide of potassium to heat, whereby ammonia is set free, contains nitruret of potassium, $K^3\,\underline{N}$.

Schrötter, to whose experiments we have above alluded, has succeeded in preparing several nitrurets, of which he has given a description, and their mode of preparation, analysis and properties, from which we extract the following:—Metallic copper is not affected by being treated with dried ammoniacal gas; but, when well-dried gaseous ammonia is passed over oxide of copper, situated in a glass tube three feet long, and the anterior portion of the tube where the gas enters gently heated, formation of water and evolution of nitrogen take place, without any reduction of the oxide being perceptible. When the under portion of the tube is more strongly heated, reduction ensues with violent disengagements of nitrogen and formation of water. In the portion of the tube gently heated, the oxide of copper is found to be partially converted into a green powder; while, in that portion subjected to higher tem-

* See Dr. Kane's paper, Phil. Mag., August 1840, S. 3, vol. xvii. p. 125.

perature, only reduced copper occurs. When the green powder is heated over a spirit-lamp, a beautiful combustion appears before red heat; at the same time it is converted with violent disengagement of nitrogen into metallic copper: this indicates that the green powder is a combination of nitrogen with copper, which is confirmed by numerous experiments. The formation of the nitruret of copper takes place long before the boiling point of linseed oil, and, in fact, at 250° C., while the reduction of the oxide of copper, by means of hydrogen, only takes place at the boiling point. The degree of division of the oxide is of much importance, and it is best to employ an oxide prepared by precipitating a hot solution of the nitrate of copper with caustic potash, and boiling. Even with this precaution it is difficult to prepare a nitruret of copper perfectly free from oxide; for, after the action of ammonia has continued for about eight hours, all formation of water ceases; the powder must then be well shaken, which must be often repeated, and towards the end of the operation triturated several times in a mortar. To produce twenty grammes of nitruret of copper, it is requisite to continue the action of ammonia during 120 hours. The hydrate or carbonate of copper cannot be employed with the same advantage as the oxide, as they become heated in contact with ammonia, and the temperature rises so high that the nascent nitruret is immediately decomposed. The nitruret of copper is a somewhat blackish, fine powder (the green powder contains still undecomposed oxide of copper), which, at a temperature of about 300°, becomes decomposed, whilst evolving a beautiful red light, into copper and nitrogen, which is mixed with nitric oxide, if the combination still contain some oxide. In carbonic acid, and in ammoniacal gas, it is decomposed at a higher, in oxygen, at a lower temperature, than in atmospheric air. It is not decomposed by pressure or friction. Acids decompose it; brought into contact with sulphuric acid, a violent disengagement of nitrogen ensues, and the copper remains behind in a metallic state. Acids which dissolve the copper, as nitric acid, act far more violently. It is impossible to free it entirely from adherent ammonia. The analysis was made according to the method generally adopted for organic nitrogenous substances. The amount of copper was determined by dissolving a portion of the combination in nitric acid, and converting the nitrate into oxide of copper. The results obtained agree exceedingly well with the formula $N\,Cu^3$*.

[* See Prof. Grove's paper on some electro-nitrogurets, in the present volume, pp. 100, 103: see also, Phil. Mag. Second Series, vol. iv. p. 155; vol. vi. p. 147.—Edit.]

Liebig observed, that on heating the bichromate of the perchloride of chromium with ammonia, or the chloride of chromium in gaseous ammonia, metallic chromium is obtained; the metal obtained, according to these two methods, exhibits, however, very different properties; the first being a black powder, which, under the steel, acquires a metallic lustre, while the latter has a dull chocolate-brown colour. This difference in the properties induced M. Schrötter to examine them more accurately: he found, that on heating the brown powder in a combustion-tube, from which atmospheric air had been expelled by means of carbonic acid, a beautiful incandescence ensued, accompanied by a violent disengagement of gas, which proved to be nitrogen mixed with nitric oxide. Another experiment showed that the substance contained no hydrogen; it is, therefore, nitruret of chromium. Its composition is represented by the formula $N^5 Cr^2$. It is impossible to free it entirely from ammonia. The brown powder is a mixture of the nitruret with the oxide of chromium, to obtain it free from which it must be prepared in the following manner:—The chloride of chromium is heated in an atmosphere of hydrochloric gas, until it is anhydrous; it has then acquired a beautiful peach-red colour, and is deposited in crystalline laminæ in the upper part of the tube. On treating it with ammonia all contact with oxygen must be carefully avoided. Thus prepared, the nitruret of chromium is black, and has all the properties of the body prepared after the other method. (Mitscherlich in *Poggendorff's Annalen*, vol. xlix. p. 407; Berzelius in *Journ. für Prakt. Chem.* vol. xxiii. p. 230; and Schrötter in *Annal. der Chem. und Pharm.* vol. xxxvii. p. 129.)

Action of Ammonia on glowing Charcoal; formation of Prussic Acid, &c.

In a notice published in Liebig's *Ann. der Chem. und Pharm.* vol. xxxviii. p. 69, M. Kuhlmann advances the following positions, to which he has been led by experiments on the properties of spongy platinum:—1. All volatile nitrogenous compounds can, when mixed with air, oxygen, or an oxygenous gas, be converted by this means into nitric acid or hyponitric acid: 2, the same compounds produce with hydrogen, or an hydrogenous gas, ammonia: 3, mixed with hydrocarbons, or when the nitrogen compound contains hydrogen with carbonic oxide, they produce prussic acid or the prussiate of ammonia. In repeating the experiment described by Clouet, viz. the production of prussic acid by the action of ammonia on incandescent charcoal, M. Kuhlmann found it to succeed perfectly, the prussiate of ammonia is obtained however, the free

acid being decomposed at the temperature at which the action takes place; *at the same time* quadricarburet of hydrogen is *evolved*. The decomposition may be expressed by the formula $3C + 2N^2H^6 = N^2C^2H^2, N^2H^6 + CH^4$. The energetic action of charcoal on ammonia may be turned to account in preparing anhydrous prussic acid. For this purpose dried ammonia is passed through a porcelain tube containing small pieces of red-hot charcoal; the escaping gases, containing much prussiate of ammonia, are conveyed into dilute sulphuric acid, warmed to about 50°; the ammonia is detained by the sulphuric acid, and only prussic acid escapes, which is condensed in a vessel surrounded by a frigorific mixture. The acid thus obtained is quite as pure as that from the decomposition of cyanide of mercury by means of hydrochloric acid. This action of carbon with ammonia may also be employed in the preparation of the ferrocyanide of potassium; the vapour of the prussiate of ammonia is passed into a solution of caustic potash, in which the hydrate of the protoxide of iron is suspended.

Similar experiments on the action of ammonia on red-hot charcoal have been published by M. Langlois in the *Annales de Chim. et de Phys. 3ième Sér.* t. i. p. 117, which agree with the above, with the exception that the gas which escapes at the formation of the prussiate of ammonia, is stated to contain neither nitrogen nor *carburetted hydrogen*, but to be pure hydrogen. To explain the decomposition M. Langlois adopts the notion, that one equiv. of ammonia, N^2H^6, becomes, on contact with incandescent charcoal, $N^2H^2 + H^4$; the two equiv. of hydrogen are then replaced by two equiv. carbon to form prussic acid, which immediately enters into combination with one equiv. of undecomposed ammonia; a portion of the hydrogen of the ammonia being replaced by carbon exactly as the oxygen of the metallic oxides by chlorine, when exposed with the latter to a high temperature. 0·114 of the salt gave 0·340 cyanide of silver, corresponding to 0·068 prussic acid, which gives the formula $N^2H^6 + C^2N^2H^2$.

Double Cyanurets of Zinc with Alkalies and Earths.

Samselius has found that these combinations may likewise be obtained, (besides in the usual moist way, by dissolving cyanide of zinc in the alkaline cyanides,) by mixing the alkaline cyanides with carbonate, or any other salt of the oxide of zinc, as long as the cyanide of zinc is redissolved, upon which the double salt is obtained on evaporation, at least, as far as may be concluded from the action with cyanide of potassium. Caustic ammonia dissolves cyanide of zinc, and soon deposits

the double salt. The double salt of barytes cannot be obtained by precipitating a solution of cyanide of zinc-potassium with acetate of barytes. It gives a white precipitate, insoluble, or nearly so, which contains barytes, with evolution of a strong smell of prussic acid. A corresponding combination with oxide of lead was obtained on precipitating cyanide of zinc-potassium with acetate of lead. It forms a white powder. Acetic acid removes the lead, leaving cyanide of zinc behind. The compound was not analysed, but regarded as $Zn\,\underline{Cy} + \dot{Pb}$. This kind of combination of a cyanide with an oxide of another metal will be new and remarkable, should it be confirmed. Cyanide of zinc-magnesium could not be prepared; the cyanogen is decomposed during evaporation, and a brown substance is deposited.—(*Berz. Jahresber.* vol. xx. part ii. p. 153.)

On the Stibio- and Arsenio-Sulphurets.

Persulphuret of antimony, $Sb^2\,S^5$, combines with basic sulphurets to form very peculiar crystallizable salts, of which, however, only the sodium and potassium compounds have been examined. Dr. Rammelsberg has published a memoir on the subject. Some chemists consider $Sb^2\,S^5$ as a mixture of $Sb^2\,S^3 + S^2$, because it is decomposed into these elements by boiling with oil of turpentine; its power, however, of forming characteristic salts, speaks for its being a determinate compound. [A fact in favour of its being a determinate sulphuret is, that when boiled with caustic soda, stibio-persulphuret of sodium and antimoniate of soda are formed, but no hyposulphite; on the other hand, the two atoms of sulphur may be driven out at a temperature by which sulphur boils; they may also be extracted by sulphuret of carbon.] (Mitscherlich, *Poggendorff's Annalen*, xlix. p. 412.)

The stibio-persulphurets may be formed in several ways. Those with the bases of the alkaline and earthy metals are soluble in water, and are either colourless or yellow; the insoluble salts are of various colours. The soluble sulphurets are decomposed by carbonic acid; the insoluble ones are often very difficult of decomposition.

The stibio-persulphuret of potassium is deliquescent; its formula is $3\,K\,S + Sb^2\,S^5 + 9\,H^2\,O$, or $\dot{K}^3\,\overset{\cdot\cdot\cdot}{\underline{Sb}} + 9\,\underline{\dot{H}}$. When sulphuretted hydrogen is passed through a solution of the neutral antimoniate of potash, persulphuret of antimony is thrown down, and the above-mentioned stibio-sulphuret remains in solution. Persulphuret of antimony loses its colour when treated with a solution of caustic potash; it dissolves,

and at the same time a heavy white powder is formed; this is bin-antimoniate of potassa, $\dot{K}\,\overset{:\cdot:}{\underline{Sb}} + 6\,\underline{\dot{H}}$. If the solution be evaporated to a certain point, and then allowed to cool, a colourless salt separates in long acicular crystals; they are not deliquescent, but soon become covered with a brown substance. They are soluble in boiling water, but decomposed by cold water, by which part is dissolved, and antimoniate of potash remains behind. This is a compound of the stibio-persulphuret with antimoniate of potash. Its formula is $(3\,K\,S + Sb^2\,S^5 + 9\,H^2\,O) + (K\,O + Sb^2\,O^5 + H^2\,O)$, or $(\overset{,}{K}{}^3\,\overset{,,,,,}{\underline{Sb}} + 9\,\underline{\dot{H}}) + (\dot{K}\,\overset{:\cdot:}{\underline{Sb}} + \underline{\dot{H}})$. With soda no such salt can be obtained.

Stibio-sulphuret of Sodium.—Schlippe's salt crystallizes in tetrahedrons; subordinate are the faces of the second tetrahedron, dodecahedron, and tetratrishexahedron $(a : \frac{1}{2}a \propto a)$. The solution of this salt, by exposure to the air, deposits a reddish-brown powder: in the solution are contained carbonate and hyposulphite of soda. As to the formula of this salt there is some degree of uncertainty. Different formulæ have been given. Duflos's is $\overset{,}{Na}{}^2\,\overset{,,,,,}{\underline{Sb}} + 8\,\underline{\dot{H}}$; Liebig's, $\overset{,}{Na}\,\overset{,,,,,}{\underline{Sb}} + 12\,\underline{\dot{H}}$; Schlippe's, $\overset{,}{Na}{}^3\,\overset{,,,,,}{\underline{Sb}} + 20\,\underline{\dot{H}}$. Rammelsberg found $\overset{,}{Na}{}^3\,\overset{,,,,,}{\underline{Sb}} + 18\,\underline{\dot{H}}$.

The barium salt can be obtained crystallized; its formula is $\overset{,}{Ba}{}^3\,\overset{,,,,,}{\underline{Sb}} + 6\,\underline{\dot{H}}$.

The ammonium, strontium, calcium and magnesium salts do not crystallize; they all, however, contain three atoms of basic sulphuret.

To obtain the pure metallic stibio-sulphurets, the solutions of the metallic salts must be added gradually to an excess of the alkaline stibio-sulphuret.

The silver, lead and copper salts, obtained in this manner, have the formulæ $\overset{,}{Ag}{}^3\,\overset{,,,,,}{\underline{Sb}}$, $\overset{,}{Pb}{}^3\,\overset{,,,,,}{\underline{Sb}}$, $\overset{,}{Cu}{}^3\,\overset{,,,,,}{\underline{Sb}}$. When the silver salt is heated sulphur is given off, and a substance remains having the formula $\overset{,}{Ag}{}^3\,\overset{,,,}{\underline{Sb}}$, the same as red silver; by heating the lead compound, $\overset{,}{Pb}{}^3\,\overset{,,,}{\underline{Sb}}$ is obtained; this is the composition of Boulangerite. With corrosive sublimate the salt $\overset{,}{Hg}{}^3\,\overset{,,,,,}{\underline{Sb}}$ may also be obtained. With sulphate of zinc an

orange-coloured body is formed; its composition is $\dot{Zn}^3\, \overset{,,,}{\underline{Sb}}$ $+ \dot{Zn}$.

When the alkaline stibio-sulphuret is added to an excess of the metallic salt, and the whole boiled for some time, a totally different kind of salt is produced. By caustic potash these salts are decomposed into metallic sulphurets and antimonic acid. The silver salt contains $Ag^8\, S^8\, Sb^2\, O^5$; it may be either $\acute{Ag}^3\, \overset{,,,}{\underline{Sb}} + 5\, \dot{Ag}$, or $8\, \acute{Ag} + \overset{\cdot\cdot\cdot\cdot\cdot}{\underline{Sb}}$. The lead and copper salts have the same constitution. With chloride of mercury a white precipitate is obtained; its composition is $\acute{Hg}^3\, \overset{,,,}{\underline{Sb}} + 3\, Hg\, Cl^2 + 3\, \dot{Hg}$.

When arsenio-sulphuret of sodium is added to an excess of sulphate of copper, and the whole boiled, the black insoluble substance which is formed is sulphuret of copper, Cu S, and in the solution is arsenic acid, containing the whole of the employed arsenic. It is therefore probable that these above-mentioned salts are compounds of the metallic sulphurets with antimonic acid. (*Poggendorff's Annalen*, lii. p. 193–242.)

XL. *Meteorological Summary of the Weather at Montreal, Province of Canada, in Lat.* 45° 30′ *N., Long.* 73° 22′ *W., from Registers kept* (*by* J. S. McCord, *Associate Member of the London Meteorological Society, Member of the Natural History Society of Montreal, Corresponding Member of the Literary and Historical Society, Quebec, and Albany Institute, New York,*) *for five years, from* 1836 *to* 1840 *inclusive**.

TEMPERATURE.				
ANNUAL TEMPERATURE.				
	Mean Temp.	Max. Temp.	Min. Temp.	Range.
1836.	40·43	90·00	−19·00	109·00
1837.	41·22	90·00	−18·00	108·00
1838.	41·58	90·00	−13·00	103·00
1839.	44·07	89·00	−18·00	109·00
1840.	44·29	91·00	−14·50	105·50
Mean.	42·31	90·00	−16·50	106·90

* Reprinted from a table privately distributed by the author, communicated by Dr. Daubeny.

TEMPERATURE.

MEAN MONTHLY TEMPERATURE.

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
1836.	17·05	11·32	20·90	35·24	53·32	65·25	71·90	63·05	57·46	39·37	32·24	18·09	
1837.	8·70	15·78	25·15	40·40	52·15	65·55	65·60	65·15	58·30	43·20	33·70	20·95	
1838.	21·00	9·00	31·60	34·50	52·80	69·90	68·90	65·50	57·90	46·60	28·80	12·40	
1839.	12·70	20·80	29·30	45·50	52·90	62·20	70·00	66·50	59·60	50·00	34·40	25·00	
1840.	10·45	23·50	30·55	44·46	55·73	66·99	71·96	70·87	59·26	46·35	34·36	17·02	
Mean.	13·98	16·08	27·50	40·02	53·38	65·97	69·67	66·21	58·50	45·10	32·70	18·69	42·316

WEATHER.

	Clear.	Cloudy.	Rain.	Snow.	Showers.	Fog.	Days observed.
1836.	151·50	134·79	34·00	25·75	10·00	1·00	357·04
1837.	135·25	132·75	34·25	13·00	9·50	2·25	327·00
1838.	146·75	112·50	32·50	16·50	13·25	0·50	322·00
1839.	167·00	118·50	43·00	13·75	10·00	6·75	359·00
1840.	141·25	138·50	38·50	21·00	11·25	5·50	356·00
Mean.	148·35	127·40	36·45	18·00	10·80	3·20	

Averaging per cent.—Clear days about 44; Cloudy, 35; Rain, 11; Snow, 5; Showery, 3; Fog, Hail, &c. 2.

PRESSURE. Corrected and reduced to 32° F.			RAIN.	WINDS.				
	Mean Ann. Pressure.	Annual Range.	Inches.	WESTERLY. N. W. W. S. W.	EASTERLY. N. E. E. S. E.	North.	South.	No. of days observed.
1836.	29·920	1·550	19.20	189·20	46·25	65·85	55·66	357·00
1837.	29·823	1·758	16·90	200·00	32·00	56·50	38·50	327·00
1838.	29·884	1·498	18·60	163·00	32·25	66·75	49·25	311·25
1839.	29·909	2·128	15.90	179·00	83·00	63·50	33·50	359·00
1840.	29·895	1·569	27·55	188·50	71·50	47·50	45·50	353·00
Mean.	29·886	1·700	19·63	183·94	53·00	60·02	44·48	
		Or about . . .		54 per cent.	15 per cent.	18 p.c.	13 p. c.	

SNOW-GAUGE.

Winter of 1830—31	73·90 inches.
31—32	107·60 ...
32—33	60·60 ...
33—34	51·00 ...
34—35	84·95 ...
35—36	86·45 ...
36—37	65·50 ...
37—38	49·85 ...
38—39	47·70 ...
39—40	49·40 ...
Mean.	67·695

These observations are all made with instruments by first (British) artists, duly compared with standards, and every precaution taken in their position to ensure the most correct indications. The city of Montreal is about 46 feet above the tide waters of the St. Lawrence; the Barometer about 45 feet above the mean level of the St. Lawrence. Total elevation of Barometer above the tide waters of the St. Lawrence, 91 feet.

XLI. *Remarks on a Statement in the* Traité de Mécanique *of* Poisson. *By* James Booth, *M.A., Principal of, and Professor of Mathematics in Bristol College**.

IN a work of great and deserved celebrity, to be found in the hands of every student of mechanical science—the *Traité de Mécanique* of Poisson—the author, in determining the three principal axes of rotation of a body, states correctly, that a principal axe passing through the centre of gravity is also a principal axe for any point assumed along this line, but seems to have committed an oversight in saying that the two remaining principal axes of the assumed point will vary in direction as the point shifts along the line. His words are, after giving the formula for determining the angle θ between the principal axe $O x'$ of the point O, and the axis of coordinates $O x$, "Les intégrales que cette équation renferme pourront changer de valeur avec la position du point O; en sorte que le long de l'axe $O z$, les deux autres axes principaux ne seront pas, en général, parallèles à eux-mêmes."—*Traité de Mécanique*, tom. ii. page 91.

The equation by which θ is determined, as given by Poisson, is

$$(\cos^2\theta - \sin^2\theta) \int x\,y\,d\,m + \sin\theta\cos\theta\,(\int x^2\,d\,m - y^2\,d\,m) = 0,$$

which may be reduced to

$$\tan 2\,\theta = \frac{2\int x\,y\,d\,m}{\int (y^2 - x^2)\,d\,m}.$$

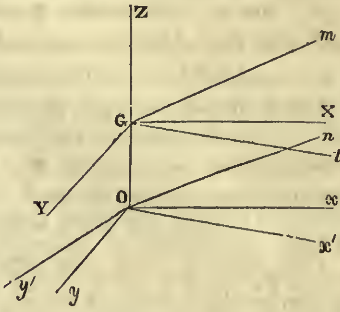
Now of the three coordinates $x\,y\,z$ of the particle $d\,m$, the only one which varies by shifting the point O along the axis of Z is z; but the above expression for the value of $\tan 2\,\theta$ is independent of z, hence the angle θ is constant, or the principal axes are always parallel for any point along the line.

A direct demonstration from established principles of the parallelism of the principal axes in this case may be given as follows:—

Assuming then that a principal axe of rotation through the centre of gravity, is also a principal axe for any point taken on this line, let G X, G Y, G Z be the three principal axes of the body passing through the centre of gravity G, O any point assumed on the axis G Z; if the three principal axes through this point are not parallel to those, through the centre of gravity, let the principal axe $O\,x'$ make with the line $O\,x$, which is parallel to the line G X, the angle ω; let the moments of inertia round the principal axes G X, G Y, G Z be

* Communicated by the Author.

A, B, C, while the moments of inertia round the principal axes $O x'$, $O y'$, $O Z$ are A', B', C; through the centre of gravity and the point O, let any two arbitrary parallel right lines be drawn, $G m$ and $O n$; let the moments of inertia round these right lines be D and D', and put the distance $G O = c$.



Let the angle $Z G m$ or $Z O n$ be θ, the angle between the planes $Z G X$ and $Z G m = \phi$, then we shall have

$$D = A \sin^2 \theta \cos^2 \phi + B \sin^2 \theta \sin^2 \phi + C \cos^2 \theta \qquad \text{(I.)}$$

But as the line $O n$ is parallel to $G m$ and distant from it by the distance $c \sin \theta$, we shall have (*Traité de Mécanique*, tom. ii. page 53.), M being the mass of the body,

$$D' = D + M c^2 \sin^2 \theta, \text{ or putting for D its value,}$$

$$\left.\begin{aligned} D' = A \sin^2 \theta \cos^2 \phi + B \sin^2 \theta \sin^2 \phi + C \cos^2 \theta \\ + M c^2 \sin^2 \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \text{(II.)}$$

The angle between the planes $Z O n$ and $Z O x'$ is $(\phi - \omega)$: referring then the moment of inertia D' to the moments of inertia of the three principal axes passing through the point O, we find

$$\left.\begin{aligned} D' = A' \sin^2 \theta \cos^2 (\phi - \omega) + B' \sin^2 \theta \sin^2 (\phi - \omega) \\ + C \cos^2 \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \text{(III.)}$$

Equating the values of D' given by equations (II.) (III.), eliminating and dividing by $\sin^2 \theta$, there results

$$\left.\begin{aligned} A \cos^2 \phi + B \sin^2 \phi + M c^2 = A' \cos^2 (\phi - \omega) \\ + B' \sin^2 (\phi - \omega) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \text{(IV.)}$$

We have now to determine A', B'.

Through G let a right line $G t$ be drawn parallel to $O x'$; the moment of inertia round this line is $A \cos^2 \omega + B \sin^2 \omega$, and as this line passing through the centre of gravity is distant from $O x'$ by c, the moment of inertia round $O x'$ or A' is equal to

$$A \cos^2 \omega + B \sin^2 \omega + M c^2. \text{ Similarly,}$$

$$B' = A \sin^2 \omega + B \cos^2 \omega + M c^2.$$

Substituting these values of A' and B' in (IV.), we find

$$\left.\begin{aligned} A \cos^2 \phi + B \sin^2 \phi = A\{\cos^2 \omega \cos^2 (\phi - \omega) + \sin^2 \omega \\ \sin^2 (\phi - \omega)\} + B\{\sin^2 \omega \cos^2 (\phi - \omega) + \cos^2 \omega \\ \sin^2 (\phi - \omega)\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \text{(V.)}$$

Now $\phi = \omega + \phi - \omega$, hence

$$\cos\phi = \cos\omega\cos(\phi-\omega) - \sin\omega\sin(\phi-\omega), \text{ or}$$

$$\cos^2\phi = \cos^2\omega\cos^2(\phi-\omega) + \sin^2\omega\sin^2(\phi-\omega) - 2\sin\omega\cos\omega\cos(\phi-\omega)\sin(\phi-\omega). \text{ Similarly,}$$

$$\sin^2\phi = \sin^2\omega\cos^2(\phi-\omega) + \cos^2\omega\sin^2(\phi-\omega) + 2\sin\omega\cos\omega\cos(\phi-\omega)\sin(\phi-\omega).$$

Putting these values of $\cos^2\phi$, $\sin^2\phi$ in the first member of (V.) and eliminating, we find

$$(A-B)\sin 2\omega \sin 2(\phi-\omega) = 0 \quad . \ . \ . \ . \quad (\text{VI.})$$

Now as ϕ is quite arbitrary, we may always assume it differing in value from ω, hence we may consider $\sin 2(\phi-\omega)$ as always different from zero; dividing then by this factor, equation (VI.) is thus reduced to

$$(A-B)\sin 2\omega = 0 \quad . \ . \ . \ . \ . \ . \ . \quad (\text{VII.})$$

This equation is satisfied when $A = B$ by any values of ω, or, in other words, when the moments of inertia round two of the principal axes passing through the centre of gravity are equal; any pair of rectangular axes in a plane perpendicular to the third principal axe are also principal axes.

When A is not equal to B, equation (VII.) can only be satisfied by the values $\omega = 0$, or $\omega = \frac{\pi}{2}$, or the principal axe $O x'$ coincides with $O x$, which is parallel to G X, hence we may deduce the following theorem:—

The principal axes of rotation of a body passing through any point assumed on one of the three principal axes through the centre of gravity, are always parallel to these axes.

XLII. *On the Focal Lengths and Aberrations of a thin Lens of Uniaxal Crystal, bounded by Surfaces which are of Revolution about its Axis. By* Sir William Rowan Hamilton, *P.R.I.A., Member of several Scientific Societies at Home and Abroad, Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland**.

THE following short investigation may perhaps be not without interest to the students of mathematical optics, as serving to illustrate a general method, and to correct an important error into which an eminent writer has fallen.

1. Let a ray of ordinary light, *in vacuo*, and in the plane of $x z$, proceeding from or towards a given point on the axis of z, be incident nearly perpendicularly on a given surface of revolution, and there undergo extraordinary refraction at en-

* Communicated by the Author.

tering a given uniaxal crystal, of which the optical axis coincides with the axis of revolution and of z. It is required to determine the intersection of the refracted ray with the axis; the distance of the point of incidence from the vertex, or the semiaperture of the crystal, being given.

2. The law of this extraordinary refraction may (according to the general methods of my 'Theory of Systems of Rays,' published in the Transactions of the Royal Irish Academy,) be thus expressed:

$$(\sigma - \alpha') \, \delta \, \xi + (\upsilon - \gamma') \, \delta \, \zeta = 0. \quad \ldots . \quad (1.)$$

In this formula, ξ, ζ are (in the plane of $x z$) the rectangular coordinates of incidence, and are connected by the equation of the meridional section of the given refracting surface of revolution, which equation we may suppose to be (at least nearly enough for our present purpose) developed under the form

$$\zeta = \frac{r}{2} \, \xi^2 + \frac{s}{4} \, \xi^4, \quad \ldots \ldots . \quad (2.)$$

r being the curvature at the vertex, and s being another constant, which in the case of a spheric surface is half of the cube of that curvature; α', γ' are the cosines of the inclinations of the incident ray to the positive semiaxes of $x z$, so that they are connected by the relation

$$\alpha'^2 + \gamma'^2 = 1 ; \quad \ldots \ldots \ldots . \quad (3.)$$

and σ, υ are the components of normal slowness of the extraordinary wave within the crystal, so that, if μ be the ordinary and ν the extraordinary index, these components are connected by the relation

$$\mu^2 \, \sigma^2 + \nu^2 \, \upsilon^2 = \mu^2 \, \nu^2. \quad \ldots \ldots \ldots . \quad (4.)$$

And these quantities, $\alpha' \; \gamma' \; \sigma \; \upsilon$, are such, that if $x' \; z'$ be the coordinates of any point on the incident ray, we have

$$(\xi - x') \, \delta \, \alpha' + (\zeta - z') \, \delta \, \gamma' = 0 ; \quad \ldots . \quad (5.)$$

and if $x \, y$ be the coordinates of any point on the refracted ray, we have

$$(x - \xi) \, \delta \, \sigma + (z - \zeta) \, \delta \, \upsilon = 0. \quad \ldots \ldots \quad (6.)$$

3. Making then

$$\mathrm{T} = \xi \, (\sigma - \alpha') + \zeta (\upsilon - \gamma'), \quad \ldots \ldots \quad (7.)$$

we have first the equation

$$\frac{\delta \, \mathrm{T}}{\delta \, \xi} = 0, \quad \ldots \ldots \ldots \ldots . \quad (8.)$$

which contains the law of extraordinary refraction, and by which ξ can be eliminated from the expression of T, so as to

leave that quantity T expressed as a function of α' and σ only; ζ being here treated as a known function of ξ, γ' of α', and υ of σ. And since, by (1.) (5.) (6.),

$$\delta \mathrm{T} = x \,\delta \sigma + z \,\delta \upsilon - x' \,\delta \alpha' - z' \,\delta \gamma', \quad \ldots\ldots \quad (9.)$$

we have, by (3.) and (4.), the following equations, for the incident and refracted rays respectively:

$$- x' + \frac{\alpha'}{\gamma'} z' = \frac{\delta \mathrm{T}}{\delta \alpha'}; \quad \ldots\ldots \quad (10.)$$

$$x - \frac{\mu^2 \sigma}{\nu^2 \upsilon} z = \frac{\delta \mathrm{T}}{\delta \sigma}. \quad \ldots\ldots \quad (11.)$$

4. The approximate equation (2.) of the section of the refracting surface, which gives ζ as an explicit function of ξ, is now to be combined with the following analogous expressions for the functions γ' and υ, deduced from the relations (3.) and (4.):

$$\gamma' = 1 - \frac{\alpha'^2}{2}; \; \upsilon = \mu - \frac{\mu \sigma^2}{2 \nu^2}; \quad \ldots\ldots \quad (12.)$$

and thus the expression (7.) for T becomes, if we neglect terms which are small of the sixth dimension with respect to α', σ, ξ:

$$\mathrm{T} \;\; = \mathrm{T}^{(2)} + \mathrm{T}^{(4)}; \quad \ldots\ldots \quad (13.)$$

$$\mathrm{T}^{(2)} = \xi (\sigma - \alpha') + \tfrac{1}{2} r \xi^2 (\mu - 1); \quad \ldots\ldots \quad (14.)$$

$$\mathrm{T}^{(4)} = \tfrac{1}{4} s \xi^4 (\mu - 1) - \tfrac{1}{4} r \xi^2 \left(\frac{\mu \sigma^2}{\nu^2} - \alpha'^2 \right) \ldots \quad (15.)$$

And to eliminate ξ, it is sufficient to employ the equation (8.) under the approximate form

$$0 = \frac{\delta \mathrm{T}^{(2)}}{\delta \xi} = \sigma - \alpha' + r \xi (\mu - 1); \quad \ldots\ldots \quad (16.)$$

for although the complete expression for the abscissa ξ of incidence contains terms of the third and higher dimensions with respect to α' and σ, yet the introduction of these terms of ξ would only introduce terms of the sixth and higher dimensions, in the expression for the function T.

5. Retaining therefore ξ as an auxiliary symbol, of which the meaning is determined by the formula (16.), and making the abscissæ x' and x to vanish in the equations of the two rays, (10.) and (11.), in order to discover the relation between the ordinates z' and z of the intersections of those two rays with the axis, we find

$$+ \alpha' z' \left(1 + \frac{\alpha'^2}{2}\right) = - \xi + \frac{\delta T^{(4)}}{\delta \alpha'}; \quad \dots\dots \quad (17.)$$

$$- \frac{\mu \sigma z}{\nu^2} \left(1 + \frac{\sigma^2}{2 \nu^2}\right) = + \xi + \frac{\delta T^{(4)}}{\delta \sigma}; \quad \dots \quad (18.)$$

and therefore, taking the reciprocals,

$$- \frac{1}{z'} = \frac{\alpha'}{\xi} \left(1 + \frac{\alpha'^2}{2}\right) + \frac{\alpha'}{\xi^2} \frac{\delta T^{(4)}}{\delta \alpha'}; \quad \dots\dots \quad (19.)$$

$$\frac{\nu^2}{\mu z} = \frac{- \sigma}{\xi} \left(1 + \frac{\sigma^2}{2 \nu^2}\right) + \frac{\sigma}{\xi^2} \frac{\delta T^{(4)}}{\delta \sigma}. \quad \dots \quad (20.)$$

Adding these last two equations, attending to the value (16.) of ξ, and observing that $T^{(4)}$, after substitution of that value, becomes a homogeneous function of the fourth dimension of α' and σ, so that

$$\alpha' \frac{\delta T^{(4)}}{\delta \alpha'} + \sigma \frac{\delta T^{(4)}}{\delta \sigma} = 4 \, T^{(4)}, \quad \dots\dots\dots\dots \quad (21.)$$

we find this relation:

$$\frac{\nu^2}{\mu z} - \frac{1}{z'} - (\mu - 1) r = \frac{1}{2 \xi} \left(\alpha'^3 - \frac{\sigma^3}{\nu^2}\right) + \frac{4}{\xi^2} T^{(4)}. \quad (22.)$$

And changing, in the second member, α' and σ to their approximate values given by (19.) and (20.), namely,

$$\alpha' = - \frac{\xi}{z'}, \quad \sigma = - \frac{\nu^2 \xi}{\mu z}, \quad \dots\dots\dots \quad (23.)$$

we find

$$\left.\begin{aligned} &\frac{\nu^2}{\mu z} - \frac{1}{z'} - (\mu - 1) r \\ &= \frac{\xi^2}{2} \left\{ \frac{\nu^4}{\mu^3 z^3} - \frac{1}{z'^3} + 2 (\mu - 1) s - 2 r \left(\frac{\nu^2}{\mu z^2} - \frac{1}{z'^2} \right) \right\}. \end{aligned}\right\} (24.)$$

By suppressing the second member, we get the intersection of the axis with a refracted ray infinitely near it, or the extraordinary focus of the central rays; by taking account of that member we get the longitudinal aberration.

6. As a verification, we may consider the case of an ordinary refraction at a spheric surface as being included in the foregoing, and the formula for that case must result from the equation (24.), by making therein

$$\nu = \mu, \quad s = \frac{1}{2} r^3. \quad \dots\dots\dots\dots\dots \quad (25.)$$

Accordingly these values give

$$\frac{\mu}{z} - \frac{1}{z'} - (\mu - 1) r$$
$$= \frac{\xi^2}{2} \left\{ \mu \left(\frac{1}{z^3} - \frac{2r}{z^2} + r^3 \right) - \left(\frac{1}{z'^3} - \frac{2r}{z'^2} + r^3 \right) \right\};$$

in the second member of which we have

$$\mu \left(\frac{1}{z^3} - \frac{2r}{z^2} + r^3 \right) = \mu \left(\frac{1}{z} - r \right) \left(\frac{1}{z^2} - \frac{r}{z} - r^2 \right),$$

$$\frac{1}{z'^3} - \frac{2r}{z'^2} + r^3 = \left(\frac{1}{z'} - r \right) \left(\frac{1}{z'^2} - \frac{r}{z'} - r^2 \right),$$

and may write

$$\mu \left(\frac{1}{z} - r \right) = \frac{1}{z'} - r = \frac{\mu}{\mu - 1} \left(\frac{1}{z'} - \frac{1}{z} \right);$$

the formula for an ordinary spheric refraction is therefore thus found to be

$$\left. \begin{aligned} &\frac{\mu}{z} - \frac{1}{z'} - (\mu - 1) r \\ &\quad = \frac{\mu}{\mu - 1} \left(r - \frac{1}{z} - \frac{1}{z'} \right) \left(\frac{1}{z} - \frac{1}{z'} \right)^2 \frac{\xi^2}{2}, \end{aligned} \right\} \quad . . (26.)$$

in which it may be remarked that $\left(\frac{1}{z} - \frac{1}{z'} \right)^2 \xi^2$ is the square of the angular deviation, and which is easily seen to agree with known results.

7. Returning to the crystal, let it be bounded by a second surface of revolution, infinitely near to the former, and about the same axis; and let the light emerge at this second surface into a vacuum again. The equation of the second surface being

$$\zeta' = \frac{1}{2} r' \xi'^2 + \frac{1}{4} s' \xi'^4, \quad . . . . \quad (27.)$$

and the ordinate of the intersection of the emergent ray with the axis being z'', the formula (24.) will apply to this new case by merely changing r, s, z', z respectively to $-r', -s', -z'', -z$, without changing μ, ν, ξ; and we have

$$\left. \begin{aligned} &\frac{-\nu^2}{\mu z} + \frac{1}{z''} + (\mu - 1) r' \\ &= \frac{\xi^2}{2} \left\{ \frac{-\nu^4}{\mu^3 z^3} + \frac{1}{z''^3} - 2(\mu - 1) s' + 2 r' \left(\frac{\nu^2}{\mu z^2} - \frac{1}{z''^2} \right) \right\}. \end{aligned} \right\} (28.)$$

And, adding the two equations (24.) and (28.), we find

$$\left.\begin{aligned} &\frac{1}{z''} - \frac{1}{z'} - (\mu - 1)(r - r') \\ &\quad = \frac{\xi^2}{2}\left\{\begin{aligned} &\frac{1}{z''^3} - \frac{1}{z'^3} + 2(\mu - 1)(s - s') \\ &+ \frac{2(r' - r)\nu^2}{\mu z^2} - \frac{2r'}{z''^2} + \frac{2r}{z'^2} \end{aligned}\right\}; \end{aligned}\right\} \quad \ldots\ldots (29.)$$

a formula for the focal lengths and aberrations of a lens of uniaxal crystal bounded by any two infinitely near surfaces, which have the optical axis of the crystal for their common axis of revolution: all cases of cusps or other singular curvatures at the common vertex being here set aside.

8. We shall content ourselves at present with drawing two conclusions from this formula. First, that because the extraordinary index ν disappears from the part unaffected with the small factor ξ^2, the central focus of the extraordinary rays, after emerging from the thin crystalline lens, coincides with the central focus of the ordinary rays which emerge from the same lens, wherever in the axis the focus of the incident rays may be: whereas Malus *, misled by an error of sign in a radical which he employed for expressing the law of extraordinary refraction in the case of an uniaxal crystal, thought that these foci, ordinary and extraordinary, might differ widely from each other. And second, that on account of the presence of ν in the term

$$\frac{(r' - r)\nu^2 \xi^2}{\mu z^2}, \quad \ldots\ldots\ldots\ldots (30.)$$

the ordinary and extraordinary aberrations cannot be exactly the same (the power $(\mu - 1)(r - r')$ of the lens being supposed to be different from 0), unless the z in the denominator of this term become infinite; that is, unless, wherever the focus of incident rays may be, the lens is so placed as to allow the rays within the crystal to be exactly parallel to the axis. If then a lens of this sort be used for the object-glass of a telescope, it seems to be desirable that its anterior surface, or that on which the parallel rays fall, should be plane, and that the correction of the aberration of figure should be effected entirely by another lens, composed of an uncrystallized material.

W. R. H.

Observatory of Trinity College, Dublin,
September 14, 1841.

* Perhaps it may not be improper to mention here, as I am not aware that the correction of Malus's result, respecting the extraordinary focal length of a lens of uniaxal crystal, has been hitherto published by any other person, that this correction occurred to me many years ago, in treating the question by my own methods, which made it scarcely possible to fall into the same error. The discrepance between my conclusion and his was, however, so great, that I was at first perplexed to account for it, until I traced that error of sign in his calculations, to which allusion has been made above.

XLIII. *Experiments on the alleged Conversion of Carbon into Silicon. By* R. H. Brett, *Ph. D., and* J. Denham Smith, *Esq.*

To Richard Phillips, Esq.

Dear Sir,

In the Transactions of the Royal Society of Edinburgh of this year, some experiments have been detailed by Dr. S. M. Brown, announcing the startling discovery, that carbon in certain states of combination is susceptible of conversion into silicon, and inviting a repetition of the trials made by the author.

Anxious to satisfy ourselves respecting the accuracy of the statements made by Dr. Brown, we instituted the subjoined experiments for the purpose of verifying the results which he details.

In his experiments Dr. Brown has made use of paracyanogen, obtained by the decomposition of the bicyanide of mercury; the paracyanogen employed by us was produced by the decomposition of hydrocyanic acid; this was of a brownish black colour, and soluble in sulphuric acid, from which it was precipitated on the addition of water. It contained traces of sulphate of lime and iron.

We have deemed it unnecessary to repeat the whole of the numerous and, in many cases, prolonged experiments given in the paper, but have selected those which bore most directly on the stated transformation; the modes of procedure and the results arrived at by us, we shall describe without explanation or comment.

In page 231 of the Transactions we find it stated, that a tightly luted Berlin porcelain crucible, filled with paracyanogen, was imbedded in stucco paste and exposed to a white heat for an hour and a half. The residue of this experiment was a dark brown infusible substance like charcoal; this, when ignited with carbonate of potash, and the product treated with hydrochloric acid, is described as leaving a fine white gritty powder insoluble in acids and alkalies, infusible in microcosmic salt, and dissolving with effervescence in fused carbonate of potash, and then forming a solution in water, from which it was separable as a bulky gelatinous precipitate by acids, which precipitate was soluble both in acids and alkalies, and insoluble in either after ignition to redness. It is further stated, that " two grains of the dark ignited substance procured by the last process yielded 4·11 grs. of silicic acid; one grain, 2·06 grs., and 0·8 gr. 1·57."

This experiment was repeated in the following manner:—50

grs. of paracyanogen, obtained by the spontaneous decomposition of prussic acid, were stuffed into a small crucible of Berlin porcelain furnished with a cover; this covered crucible was completely imbedded in plaster of Paris made into a paste with water, in a Hessian crucible; a cover was then placed over the latter and luted with China clay.

This apparatus was placed in a sand-bath to dry, and afterwards submitted for three hours to a full red heat; when cold, the smaller crucible was carefully opened; there was found a perfectly black carbonaceous-looking residue, weighing 4 grs.; this black residue was ignited in a platinum capsule over a gas flame, and there remained a very inconsiderable light and grayish-coloured ash, which, when boiled in aqua regia, entirely dissolved. The ash in question consisted of sulphate of lime and traces of oxide of iron.—(R. H. B.)

Again, 50 grs. of paracyanogen were placed in a porcelain crucible under precisely similar circumstances to the last experiment. The crucibles in this case were exposed to a full white heat for two hours. When the smaller crucible was opened, a black residue, like that obtained in the former experiment, was found; this residue, when burnt in a platinum crucible with access of air, was consumed, with the exception of a trifling ash entirely soluble in aqua regia, and consisting of sulphate of lime and oxide of iron, as in the former experiment.—(R. H. B.)

Not content with the results of the two experiments just detailed, 50 grs. of paracyanogen were again subjected to an intense white heat in a furnace capable of fusing metallic iron and manganese, having previously rammed it tightly into a Berlin crucible, to which was adapted a suitable cover; these were luted together very carefully; when the luting, which then presented no cracks, was dry, this crucible was placed in an earthen one, and the vacant space filled up with a ferruginous sand; this again was covered and securely luted; after ignition for two hours it was withdrawn, when the residuum proved to be a perfectly black substance, exactly resembling that mentioned in the former experiments, weighing about 5 grs. The sand had agglomerated into a solid mass resembling soft white sandstone, and owing to a portion of this getting mixed with the black residue in the crucible, the exact weight could not be ascertained. The interior glaze of the crucible, to the level of the original bulk of the paracyanogen, had assumed a black colour with a shade of brown, which apparently penetrated to the depth of the glaze, but no further; on exposure to long-continued heat with free access of air, this black lining underwent but a very slight, if any,

change of tint. Having carefully selected 3·5 grs. of the black residue so as to procure it free from the accidental admixture of sand, they were mixed with 20 grs. of carbonate of potash prepared from the bitartrate and free from silica; this mixture, which was perfectly black, was ignited for half an hour in a closed platinum crucible, which was itself placed in a covered earthen crucible carefully luted; the fused residue was black, and when treated with water left a black insoluble powder; this, washed, collected, dried, and ignited in a platinum capsule and in contact with air, glowed like ignited carbonaceous matter, and left an ash-coloured residue, which, treated with chlorate and carbonate of potash, gave a fused residuum entirely soluble in water; this solution, evaporated to dryness with the addition of hydrochloric acid in excess, and subsequently ignited, gave a slightly turbid solution with water, which became perfectly clear on the addition of hydrochloric acid; this solution contained traces of platinum. The first solution of carbonate of potash from the black residue treated in the manner described, gave not the least indication of silica.—(J. D. S.)

Having observed that the black lining of the crucible was precisely similar to the description given by Dr. Brown of the films of silicon mentioned by him as lining the porcelain crucible in which his experiment was made, and which process he recommends as likely "eventually to be an œconomical and convenient way of lining porcelain with silicon for experimental purposes," we were desirous of ascertaining whether a cheaper and more easily to be obtained substance containing carbon, could not be advantageously substituted for paracyanogen, so as to produce the desired object; we therefore placed some lamp-black, mixed with a trace of peroxide of iron, in a Berlin crucible, into which it was tightly rammed, and exposed this crucible, having previously put it into an exterior one, which was covered and luted, to a white heat for an hour and a half: when withdrawn it was found to be coated, as high as the lamp-black had reached, with a stain, differing only from that before alluded to as obtained from paracyanogen, in being quite black; this crucible was exposed to a red heat with free access of air for upwards of two hours, but the stain remained unchanged.—(J. D. S.)

We do not say that this is not silicon, but if it be, either lamp-black is as capable of lining crucibles with silicon as paracyanogen is, or at high temperatures the carbon is capable of reducing the silica of the glaze, and thus producing the same effect.

In page 235 Dr. Brown observes, that "before leaving the production of silicon from uncombined paracyanogen, there is another mode of operating to be mentioned, and it is equally remarkable for simplicity and freedom from any intelligible source of fallacy." "Triturate crude paracyanogen with an excess of carbonate of potash, and fuse the mixture two hours at a full white heat in a closed platinum crucible. Paracyanogen disappears; there is no free carbon in the white saline product; but it yields a conformable proportion of silicic acid when treated in the ordinary method of analysis for that compound." "This process is more striking when subborate of soda is substituted for potassa; for when the product is treated with acids, there is no effervescence of carbonic acid; and it must be remembered, once for all, that in every professed process of transformation, the disappearance of carbon has to be accounted for as well as the new formation of silicon." Dr. Brown further states, that his crude paracyanogen contains "nearly a third of its own weight of condensed cyanogen," and "that it yields a weight of silicon never less than an eleventh, and never more than a twelfth, under the calculable weight of the constituent carbon, the cyanogen of absorption being dissipated in the course of the processes;" by which we presume is meant, that from crude paracyanogen he always obtained between 27·5 and 28 per cent. of silicon, a proportion so large that, when converted into silicic acid, it is impossible it could escape observation, even when the quantities of paracyanogen subjected to experiment are very small.

As the two processes just quoted are strongly recommended by Dr. Brown for their "simplicity and freedom from any intelligible source of fallacy," and as they appeared to us to bear so directly upon the question of transformation as to be absolutely crucial experiments, we have followed the formula detailed by Dr. Brown with the greatest exactitude and care in the following experiments:—

Fifteen grs. of paracyanogen were mixed with 100 grs. of carbonate of potash prepared from the bitartrate: the mixture, placed in a closely covered platinum crucible, and this put into an earthen one, which was covered and carefully luted, was ignited at a full yellow heat for two hours; on examination when cold, a portion of the salt, probably cyanide of potassium, was found to have volatilized, cementing the cover to the platinum crucible, and the residual salt was white; this, treated with distilled water, gave a slightly opalescent solution, which, when a small portion was treated with a mixture of per- and protosulphate of iron and hydrochloric acid, gave an abundant precipitate of Prussian blue; to the solution,

hydrochloric acid was added in excess, which occasioned the evolution of the characteristic odour of hydrocyanic acid; this acid solution, evaporated to dryness, ignited to redness, and treated with water, left a trace of reddish matter entirely soluble in hydrochloric acid.—(J. D. S.)

Five grs. of paracyanogen were then mixed with about 40 grs. of dried borax, and exposed to a white heat for full two hours in a platinum crucible, covered and placed in a Hessian-luted crucible, as in the preceding experiment; when withdrawn, the Hessian crucible was completely glazed exteriorly, and had evidently been softened by the heat; in the platinum crucible there remained a glassy substance of a light sea-green colour; this, treated with water, gave an alkaline solution, which *precipitated lime-water and effervesced with hydrochloric acid*, evaporated the acid solution to dryness and fused the residue; again treated the residue with hydrochloric acid, and after evaporation to dryness, fused; the fused mass, treated with distilled water and hydrochloric acid, left a slight sediment; when this sediment was fused with carbonate of potash and the residue treated with hydrochloric acid, evaporated to dryness and ignited, the ignited chloride of potassium was wholly soluble, without any sediment whatever, in water acidulated with hydrochloric acid; the solution contained traces of lime.—(J. D. S.)

This experiment was repeated upon rather a smaller scale, with precisely the same results.

Notwithstanding the decisive nature of the experiments just detailed, we determined, in order to avoid every objection, to subject paracyanogen prepared from bicyanide of mercury—the paracyanogen employed by Dr. Brown—to the process indicated by him as simple and free from fallacy. Some bicyanide of mercury was powdered and shaken down closely in a porcelain crucible, which was then covered and firmly luted; this was placed in another crucible, also covered and subjected to a full red heat for half an hour; the residue was black and coherent; a portion of this ignited left a trace of oxide of iron, and it was almost entirely soluble in sulphuric acid.

Twenty grs. of this paracyanogen were mixed with 120 grs. of carbonate of potash and ignited for one hour and a half at a full yellow heat, the same precautions being taken as in the experiment before cited; when withdrawn, the residual salt was white, soluble in distilled water, gave with mixed per- and protosulphate of iron and hydrochloric acid a precipitate of Prussian blue, and evolved much hydrocyanic acid on the addition of hydrochloric acid, which occasioned no precipi

tate: this acid solution, evaporated to dryness, ignited, and again treated with hydrochloric acid and water, left no perceptible residue; whereas, from the statement of Dr. Brown (p. 236), if rightly understood by us, we ought to have obtained at least 11 grs. of silicic acid from this quantity of paracyanogen.—(J. D. S.)

We may here be allowed to remark, in explanation of the circumstance which seems to Dr. Brown so difficult to account for in the experiments just described, viz. "the disappearance of the carbon" of the paracyanogen when ignited with carbonate of potash, that it is probable that one portion of the paracyanogen decomposes the carbonate of potash forming carbonic oxide, mixed perhaps with carbonic acid, azote and potassium, and that the last-named substance combines with that portion of paracyanogen which is decomposed by heat into cyanogen, forming cyanide of potassium, a portion of which is volatilized, whilst, as we have seen, the remainder is found mixed with the excess of carbonate of potash used in the process: other explanations may be given, but this appears to us to be both the most feasible and the most in accordance with the facts we have noticed.

Although the foregoing experiments were as direct and, to us, as decisive as we could desire respecting the alleged transformation of carbon, we nevertheless determined to pursue the investigation further, so as to render it complete; we therefore turned our attention to the experiments of Dr. Brown (p. 236–7) on the formation of "compounds of silicon with copper, iron, and platinum, by the reaction of paracyanogen on these metals."

Three grains of paracyanogen from decomposed prussic acid were tightly wrapped up in platinum foil and placed in a porcelain crucible, the bottom and sides of which were lined with platinum foil, so as to prevent the foil containing the paracyanogen from coming in direct contact with the glazed lining of the crucible; a cover was placed over the crucible, and the whole imbedded in plaster of Paris paste contained in a Hessian crucible; when dry, the apparatus was exposed to a white heat for two hours. The plaster of Paris had become vitrified by heat, and a portion of this vitrified matter found entrance into the porcelain crucible; the latter, however, when broken up, displayed the platinum of a brilliant colour, and of softer texture than foil not previously exposed to so high a temperature. It could hardly therefore be supposed that the platinum had been acted upon by silicon, which renders it brittle, and does not render the metal more brilliant; it was obviously useless to act upon the platinum by aqua regia for

the purpose of determining the quantity of silicon, if any, which it might contain, from the fact just noticed of vitrified matter having got accidentally into the crucible.—(R. H. B.)

Twenty grains of precipitated copper were mixed with five grains of paracyanogen, and placed in a covered and luted porcelain crucible; this was placed in another crucible, also covered and carefully luted, and the apparatus exposed to a white heat for one hour and a half; when opened, the contents consisted of numerous metallic globules, mixed with a black powder; these globules were malleable, and wholly dissolved in nitric acid, whilst the black powder was consumed when ignited in contact with atmospheric air.—(J. D. S.)

Another experiment was made by rolling up some very thin sheet-copper, so as to form a tube of considerable thickness, into which five grains of paracyanogen were tightly rammed, and each extremity of the tube secured by hammering; this was placed in an earthen crucible, covered and tightly luted, along with a platinum tube made of foil filled with paracyanogen, in the same way as the copper one above described, and exposed to a bright yellow heat for one hour and a half; when the crucible was withdrawn and opened, the contents were found to be fused into a button, which was only slightly malleable, breaking readily; having removed the exterior of this button, which adhered to the crucible, it was treated with nitric acid, which left a residue having the appearance of gray metallic particles mixed with black carbonaceous matter; by ignition with access of air the black powder burnt away, leaving a gray residue entirely soluble in nitro-muriatic acid.—(J. D. S.)

Having hitherto so completely failed in our endeavours to obtain results similar to those detailed by Dr. Brown in the paper under consideration, we resolved to conclude our experimental examination of it, by endeavouring to procure silicic acid, either with or without the admixture of carbonate of potash, from ferrocyanide of potassium. These experiments are not so satisfactory as the preceding ones, owing to the energetic action of the mixed ferrocyanide and carbonate upon wrought iron at the high temperatures, but are nevertheless useful as serving to confirm the results we had previously arrived at.

An experiment was made upon ferrocyanide of potassium, by drying it carefully, and pressing 700 grains of it into a covered porcelain crucible, which was then carefully luted, and when dry plunged into a paste of plaster of Paris contained in a Hessian crucible; this was well dried on a sand-bath, then covered, luted, and exposed to a white heat for five hours; when withdrawn from the fire and opened whilst still warm,

some portions of the contents glowed like a pyrophorus; the sulphate of lime had fused down, and had acted on the exterior of the porcelain crucible; the contents consisted of a light sooty spongy mass, very friable, and containing small globules of a metallic character, whilst the crucible itself was lined with a black enamel, similar to the stain noticed in the former experiments. Nine grains of this residue, from which the metallic globules had been separated, were mixed with 30 grains of nitrate of potash, and the same weight of carbonate of potash prepared from the bicarbonate, and the mixture fused in a platinum crucible; when cold, the fused mass, treated with water, afforded a solution of a deep purple colour, exactly resembling permanganate of potash; by rapid filtration this solution was obtained clear; decomposition appeared to be going on during filtration, for the liquid whilst on the filter was in a state of slight effervescence from the escape of a gas; by exposure to atmospheric air this solution rapidly decomposed, gradually losing its pink colour, becoming turbid, and finally deposited a reddish brown powder, leaving a colourless solution; this brown powder proved on examination to be peroxide of iron without a trace of manganese*.

The colourless and alkaline solution, tested in the usual way for silica, afforded a trace of this substance, which having reason to believe resulted from impurity in the carbonate of potash used in this experiment, it was tested for silica, and found to contain apparently the same quantity as that obtained from the alkaline solution just noticed; in each case the quantity was so minute as not to affect the most delicate balance, though appreciable by the eye. The oxide of iron, separated by filtration from the purple solution, and tested in the usual way for silica, did not afford the slightest trace of this substance.

The metallic globules before noticed were treated with hydrochloric and nitric acids; a black residue was obtained, which, ignited, left a light brown ash soluble in hydrochloric acid.—(J. D. S.)

Two hundred grains of carbonate of potash, heated in an iron tube to redness for three hours, and tested in the usual way for silica, gave 0·3 grain of this substance; when the quantity

* I believe that this combination of oxide of iron with an alkali has before been noticed, but I cannot remember the authority, nor do I think any investigation of the fact was attempted. It appears to me clearly to indicate the existence of iron in a state of oxidation, doubtless higher than any we are yet acquainted with, analogous to the acid oxides of manganese. I shall pursue this investigation, and endeavour to isolate, or at least ascertain the nature and composition of, this apparent *ferric acid*(?).—J. D. S. [See *Journal de Pharmacie*, tom. xxvii. p. 97.—Ed.]

of silica contained in the carbonate of potash was thus ascertained, 200 grains of it were mixed with 50 grains of dry prussiate of potash, and ignited to redness in the same iron tube, closed with an iron plug and luted with China clay, for three hours; when cool, the contents of the tube were washed out with water and a little hydrochloric acid; a powerful odour of prussic acid was observed, and when the hydrochloric acid had been added to supersaturation, abundance of Prussian blue was formed; this was evaporated to dryness, and pure nitric acid added to the residue, the mixture transferred to a flask and heated; when the Prussian blue was entirely decomposed, the contents of the flask were evaporated to dryness, ignited, and boiled with water and hydrochloric acid; the colourless insoluble matter collected, dried and ignited, proved to be silica, and weighed 0·33 grain, showing a gain of 0·03 grain of silica, a quantity so small as readily to have been originally mixed with the 50 grains of ferrocyanide of potassium, or to be merely the error of experiment.—(R. H. B.)

When 30 grains of dried ferrocyanide of potassium were mixed with four times its weight of pure carbonate of potash and ignited in an iron box, inclosed in a closely covered and luted earthen crucible, to a white heat for three hours, the iron box was found on examination to have fused, and this fused iron quite malleable: this experiment was of course spoilt, and is merely mentioned to show the heat employed by us when we speak of a *white* heat.—(J. D. S.)

Another experiment, with twice the quantities of ferrocyanide and carbonate employed in the last experiment, and exposed to a full yellow heat for three hours, also failed from the energetic action of the fused contents on the iron box, by which the bottom of the box was corroded and perforated. —(J. D. S.)

Another experiment was tried upon 150 and 600 grains of the ferrocyanide and carbonate respectively, and ignited in a gun-barrel, protected by luting and well secured at the orifice, for six hours, at a full yellow heat. Notwithstanding the precaution of luting, the gun-barrel was corroded, and evidently from the action of the fused materials, and thus a great portion of the contents were lost; what remained was washed out by digesting water upon it for twenty-four hours, and the black insoluble matter separated from the alkaline solution, which latter evolved a strong odour of hydrocyanic acid on the addition of hydrochloric acid; the black residue, when ignited, was converted into a reddish brown powder, entirely soluble in hydrochloric acid: when these two solutions were evaporated together to dryness, and the Prussian blue formed decomposed by

nitric acid, the residuum was fused and then treated with hydrochloric acid, which left a trace of insoluble matter, probably silica, but which was so minute as not to affect a balance sensible to $\frac{1}{1000}$ gr.; this minute quantity was most probably accidental, and very probably was derived from the iron tube which was corroded. The weight of the residual chlorides, when evaporated to dryness, was 104 grs., so that if we admit that only one-seventh of the quantities originally employed were tested for silica, we should have above 20 grs. of ferrocyanide of potassium submitted to this trial, which, according to Dr. Brown (p. 245), should yield 3·6 grs. of silicic acid.—(J. D. S.)

Subsequently to these experiments, it was observed in two or three instances, that when paracyanogen made from the bicyanide of mercury was submitted to the blowpipe flame in a tube of common flint-glass, closed at one end and drawn to a capillary at the other, that gaseous matter was evolved, and a small hole blown out at that end containing the paracyanogen, through which aperture the gas escaped, and invariably burnt with the characteristic flame of cyanogen.—(R. H. B.)

This experiment was followed by placing, in a tube of Bohemian glass, having a bulb at one extremity, a small quantity of paracyanogen, prepared by dissolving the dark-coloured product which results from the spontaneous decomposition of hydrocyanic acid in sulphuric acid, and precipitating by water, well washing, and drying at a temperature of 260° to 300° Fahr., and plugging up the opening of the tube with plaster of Paris. After causing the blowpipe flame to play upon the bulb containing the paracyanogen for a short time, a small hole was blown out as before, and cyanogen continued to burn for some time.

Some more paracyanogen, prepared in a similar manner, was placed in a similar tube of Bohemian glass, but in this instance the bulb was carefully coated with luting, the open extremity plugged as before, and the apparatus carefully dried; the flame of the blowpipe was then applied to the bulb for nearly half an hour without any aperture being made in it. When the contents of the bulb were removed, they presented precisely the appearance of precipitated peroxide of iron immediately after ignition, viz. a dark black glistening aspect; this residue, when burnt in a platinum capsule, was entirely dissipated, leaving no residue whatever.—(R. H. B.)

The disengagement of cyanogen in the last experiment but one is completely at variance with the statement of Dr. Brown in his paper 'On Paracyanogen,' p. 168, "that *pure* paracyanogen (precipitated by atmospheric moisture from the sulphuric acid solution of the common product) does not afford the

slightest appearance of cyanogen, warrants the conclusion that paracyanogen once formed from cyanogen or its elements cannot be rechanged into cyanogen by heat." For it can hardly, we apprehend, be argued that the paracyanogen employed in our experiment, prepared from the crude product resulting from the decomposition of hydrocyanic acid, solution in sulphuric acid, and precipitation by water, is less pure than that obtained from paracyanogen procured from bicyanide of mercury, dissolved in sulphuric acid, and gradually precipitated by the absorption of atmospheric moisture; nor can we see how it can be argued that the failure in effecting the transformation of carbon into silicon, in the experiment just detailed, can be attributed to the employment of an impure paracyanogen, or to a substance used not being paracyanogen, because it was not obtained by the action of heat on bicyanide of mercury.

As our object in performing the experiments we have detailed was simply an inquiry respecting the fact of the statement made by Dr. Brown, of the conversion of the carbon of paracyanogen into silicon, we have not entered into a critical examination of the paper, nor do we attempt to offer any explanation of the results arrived at by us in every one of our experiments (that *the carbon of paracyanogen is incapable*, by such of the processes as we have tried, which are recommended by Dr. Brown, *of conversion into silicon*), being in every instance in complete opposition to those published by that author. We leave those interested in this subject to decide whether Dr. Brown's experiments or ours are correct, and conclude this examination by stating that the terms "white heat" and "yellow heat," when employed by us, mean respectively temperatures at which malleable iron and manganese fuse in the first case, and at which copper melts in the second; whereas Dr. Brown's white heat is a temperature evidently below our yellow heat, as he speaks of exposing a copper tube to a white heat for upwards of an hour, and does not mention the fusion of the "gypsum" when kept at a white heat for one hour and a half.

Liverpool, Sept. 17, 1841.

XLIV. *Solution of a Geometrical Problem on the Form of the Black Cross in Biaxal Crystals.*

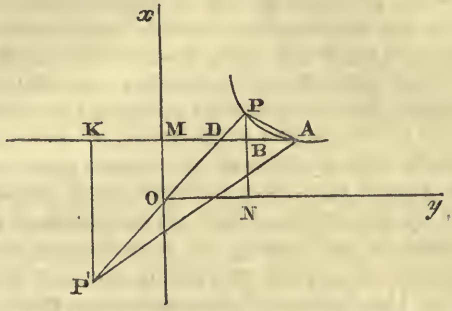
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In turning over some old papers, I have fallen upon the following very simple method of solving a geometrical problem which occurs in investigating the form of the black cross

in biaxal crystals. Sir J. Herschel has given a solution in Art. 1072 of his 'Treatise on Light,' but which, in my opinion, is unnecessarily tedious and prolix.

Problem.—Required a curve P A, such that a line drawn from A bisecting the < between lines A P, A P′ drawn to two given points P, P′, shall always be ⊥ar to a given line x O.



Solution.—Let $OP = OP' = a$, $OM = x$, $MA = y$, $PAD = P'AD = \theta$, $DOM = \alpha$.

By Euclid,

$$\frac{AP'}{AP} = \frac{P'D}{PD} = \frac{a + \dfrac{x}{\cos\alpha}}{a - \dfrac{x}{\cos\alpha}} = \frac{a\cos\alpha + x}{a\cos\alpha - x} \quad . \quad . \quad . \quad . \quad (1.)$$

Also, $KA = AP'\cos\theta = KM + MA = a\sin\alpha + y$

$AB = AP\cos\theta = AM - MB = y - a\sin\alpha$;

$$\therefore \frac{AP'}{AP} = \frac{a\sin\alpha + y}{y - a\sin\alpha} = \frac{a\cos\alpha + x}{a\cos\alpha - x} \text{ by (1.)}$$

$$\therefore \frac{y}{a\sin\alpha} = \frac{a\cos\alpha}{x}, \text{ or } xy = a^2\sin\alpha\cos\alpha = \frac{a^2}{2}\sin 2\alpha.$$

Trinity College, Cambridge, Sept. 16, 1841. J. E.

XLV. *On Malic Acid, and the Changes undergone by its Salts at High Temperatures.* *By* Robert Hagen, *Ph. D.**

AFTER the publication of Mr. Graham's observation, that phosphoric acid, in its different states, possesses the property of combining with one, with two, and with three atoms of base respectively, it was discovered by Liebig that the same

* Read before the Chemical Society, June 1, 1841, having been translated and communicated by T. G. Tilley, Esq., now communicated by the Society.

law holds good in part with many organic acids; some of these acids requiring two, and some three atoms of base, to form neutral salts. The hydrates of each acid contain a corresponding number of atoms of basic water, which cannot be removed without the decomposition of the acids themselves. These chemists showed that, though a salt of any acid, with magnesia or with oxides isomorphous with it, possessed the power of combining with a potash salt of the same acid, and forming a double salt, such as the sulphate of magnesia and potash, that is not ground for doubling the atomic weight of the acid, or for viewing it as bibasic.

They proved at the same time that a monobasic acid is incapable of forming a double salt with two isomorphous bases. The proportion of base which unites with a polybasic acid is constant, generally either two or three atoms. In the memoir on organic acids by Liebig, here alluded to, he had made it not improbable that *malic acid* is bibasic. At his suggestion I have made several analyses of its various salts, which form the subject of this paper.

Malic acid was first discovered by Scheele in the juice of the apple; it was again discovered by Donovan * in the juice of several plants, and described by him as a new acid. The identity of the acid of Donovan with malic acid was proved by Braconnot. This acid has been most fully described by Liebig. It has also been partially examined by Pelouze, Braconnot, and Richardson.

The malic acid used in the present investigations was prepared from the expressed juice of the berries of the *Sorbus aucuparia*, or Service tree, in the following manner. The expressed juice was mixed in a copper pan with finely divided and levigated hydrate of lime, care being taken not to saturate the fluid completely, but to allow it to remain sensibly sour. Being placed on the fire, it was made to boil for some hours, during which time it gave off a peculiar pungent vapour, which strongly affects the eyes. By degrees neutral malate of lime precipitates, and may be removed with a ladle. By continued boiling, more of the salt is obtained. When no more falls, the vessel is removed from the fire, and allowed to cool, when a little more is precipitated. We must take care in the beginning not to saturate the expressed juice entirely with lime, or so much colouring matter falls with the malate of lime, as to render the acid impure. The neutral malate of lime thus obtained, is dissolved in dilute nitric acid (1 part acid to 10 of water), filtered and evaporated; upon

[* Mr. Donovan's paper was reprinted from the Phil. Trans. in Phil. Mag., first series, vol. xlvi. p. 433.]

cooling, acid malate of lime crystallizes out in perfectly colourless crystals. It must be well washed with cold water, again dissolved in boiling water, and precipitated by acetate of lead. The lead salt is decomposed by sulphuretted hydrogen, and the malic acid obtained pure by evaporation.

Malic acid forms with bases two neutral salts, one of which becomes anhydrous when dried at 100° C., while the other still retains water at that temperature. It possesses decided bibasic properties, and the hitherto received atomic weight is necessarily doubled.

The following salts have been examined:—

Malates of Lime.

a. Neutral anhydrous Malate of Lime.

$$C_8 H_4 O_8 + 2\ Ca\ O, \text{ or } M\ 2\ Ca\ O.$$

This salt is obtained by saturating a solution of malic acid with lime water. It is a crystalline powder, perfectly insoluble in hot and cold water.

0·489 gramme of this salt gave 0·319 sulphate of lime, or 32·188 per cent. lime. This gives for the atomic weight of the salt the number 2212·40.

		Calculated.	Found.
1 equivalent of Malic acid	1461·39	67·24	67·81
2 equivalents of Lime . . .	712·04	32·76	32·18

b. Neutral hydrated Malate of Lime.

$$\overline{M}\ 2\ Ca\ O + 5\ aq.$$
$$\overline{M}\ 2\ Ca\ O + 4\ aq.\ (100°\ C.)$$

If acid malate of lime is saturated with potash, soda, or ammonia, and the solution evaporated at a gentle temperature, we obtain instead of a double salt, malate of lime with 5 equivalents of water, in hard shining crystals. When heated to 100° C. this salt is converted into a porcellanous mass, and is found to have lost one atom of water. At 150° C. it becomes quite anhydrous. Of the salt in its first state of hydration, 0·422 gramme dried at the temperature of the atmosphere, gave 0·2655 sulphate of lime, or 26·113 per cent. lime, which makes the atomic weight of the salt 2725·0, and gives the following composition:—

		Calculated.	Found.
1 eq. Malic Acid	1461·39	53·44	
2 ... Lime . . .	712·04	26·03	26·113
5 ... Water. . .	562·40	20·53	
	2735·83	100·00	

Of the salt dried at 100°:—

(1.) 0·6135 gramme gave 0·4045 sulphate of lime = 27·383 per cent. lime; and consequently the atomic weight, 2600·0.

(2.) 0·3660 gramme gave sulph. lime 0·241, or 27·344 per cent. lime.

(3.) 0·335 salt, gave 0·353 sulphate lime, and consequently the atomic weight, 2598·28.

These give—

			Calculated.	Found. 1.	2.	3.
1 Malic acid	1461·39		56·71			
2 Lime . . .	712·04		27·14	27·38	27·34	27·40
4 Water . .	449·92		17·15			
	2623·35					

The previous analyses of the lime salt dried without heat, shows the necessity of doubling the atomic weight of the acid, as otherwise we should be obliged to give it the formula

$$C^4 H^2 O^4 Ca O + 2\tfrac{1}{2} aq.;$$

which is at variance with the atomic theory.

Acid Malate of Lime. $\overline{M}$ Ca O H^2 O + 6 aq.

This salt is obtained when neutral malate of lime is dissolved in nitric acid. It crystallizes in large transparent octahedrons. Dried at 100° C. it loses water, and is converted into a viscid, stringy mass.

0·706 salt gave 0·2345 sulphate of lime, or 13·794 per cent. lime; atomic weight, 2585·8.

		Calculated.	Found.
1 atom Malic acid	1461·39	56·10	
1 atom Lime . . .	356·02	13·67	13·79
7 Water	787·36	30·23	
	2604·77		

Richardson and Merydorf concluded the formula of this salt to be, $\overline{M}$ Ca O H^2 O + 8 aq.

Malates of Magnesia.

a. Neutral hydrated Malate of Magnesia.

$$\overline{M}\ 2\,Mg\,O + 10\,aq$$
$$\overline{M}\ 2\,Mg\,O + 2\,aq\ (100°\ C.).$$

This salt is obtained by boiling magnesia in a solution of malic acid, and crystallizing. It loses 8 atoms of water by 100° C.

0·5505 salt gave 0·2708 sulphate of magnesia, equivalent to 16·713 per cent. of magnesia; and makes the atomic weight 3091·4.

		Calculated.	Found.
1 eq. Malic acid	1461·39	47·09	
2 ... Magnesia..	516·70	16·66	16·713
10 ... Water. . .	1124·8	36·55	
	3102·89		

Of the salt dried at 100° C., 0·466 gave 0·109 sulphate of magnesia, equivalent to 23·390° per cent. magnesia.

		Calculated.	Found.
1 at. Malic acid	1461·39	66·34	
2 ... Magnesia .	516·70	23·45	23·39
2 ... Water . .	224·96	10·21	
	2203·05		

This salt was also analysed by Professor Liebig with the same result.

b. Neutral anhydrous Malate of Magnesia. $\overline{M}$ 2 Mg O.

This salt is obtained by precipitating a saturated solution of the former salt (*a.*) with alcohol, and drying at 100° C.

0·344 salt gave 0·0935 magnesia, equivalent to 26·945 per cent.; atomic weight, 1906·88.

		Calculated.	Found.
1 atom Malic acid	1461·39	73·83	
2 atoms Magnesia	516·70	26·12	26·94
	1978·09		

Acid Malate of Magnesia. $\overline{M}$ Mg O H^2O + 3 aq
$\overline{M}$ Mg O H^2O + aq (100°).

Obtained by dividing and saturating one half of the malic acid with carbonate of magnesia, and evaporating to crystallization. It loses 2 atoms of water by 100° C.; at a higher temperature it melts.

1·1755 salt gave 0·1405 magnesia, equivalent to 11·952 per cent.; and for the atomic weight, 2161·5.

		Calculated.	Found.
1 atom Malic acid	1461·39	67·35	
1 ... Magnesia	258·35	11·91	11·952
4 atoms Water ...	449·91	20·74	
	2169·66		

Of the salt dried at 100° C., 0·698 gave 0·0795 magnesia, or 13·294 per cent.; atomic weight, 1943·0.

			Calculated.	Found.
1 eq.	Malic acid	1461·39	75·147	
1 ...	Magnesia	258·35	13·285	13·294
2 ...	Water . .	224·96	11·568	
		1944·70		

MALATES OF ZINC.

a. Neutral Salt.

$$\overline{\mathrm{M}}\ 2\,\mathrm{Z\,O} + 6\,\mathrm{aq}$$
$$\overline{\mathrm{M}}\ 2\,\mathrm{Z\,O} + - (100°\ \mathrm{C.}).$$

Is prepared by digesting carbonate of zinc with malic acid, at a temperature not above 30° C. Dried at 100° C. it becomes anhydrous.

Of this salt, 0·4570 gave 0·2935 sulphate of zinc, or 32·179 oxide of zinc, making the atomic weight 3127·8; the calculated one being 3142·77.

0·695 of the salt dried at 100° C. gave 0·566 sulphate of zinc, or 40·302 oxide of zinc; making the atomic weight 2463·6; the calculated is 2467·6.

ACID MALATE OF ZINC. $\overline{\mathrm{M}}.\ \mathrm{Z\,O\,H\,O} + 2\,\mathrm{aq}.$

Prepared by adding excess of malic acid to the neutral salt.

0·190 of this salt gave 0·082 sulphate of zinc, or 21·343 per cent. of oxide of zinc; the atomic weight deduced from which is 2351·7. The composition of the salt is therefore—

			Calculated.	Found.
1 eq.	Malic acid . .	1461·4	63·480	
1 ...	Oxide of zinc	503·2	21·861	21·343
3 ...	Water	337·4	14·659	
		2302·0		

Braconnot analysed this salt with the same results.

BASIC MALATE OF ZINC.

If malic acid is long boiled with excess of carbonate of zinc, there falls down a sandy powder; of this salt dried at 100°,

(1.) 0·3935 gave 0·178, or 44·66 per cent. of oxide of zinc; atomic weight, 2253·3.

(2.) 0·255 gave 0·224 sulphate of zinc, or 44·015 per cent. of oxide of zinc; atomic weight, 2286·8.

(1.) 0·474, burnt with oxide of copper, gave 0·1075 water, and 0·329 carbonic acid.

(2.) 0·5510 gave water 0·1335, and 0·3835 carbonic acid. This salt is therefore composed of

	Calculated.		Found.	
12 at. Carbon . . .	917·22	20·19	19·191	19·24
9 ... Hydrogen. .	112·32	2·47	2·52	2·69
15 ... Oxygen . . .	1500·00	33·03	33·62	34·04
4 ... Oxide of zinc	2012·9	44·31	44·66	44·015
	4542·44			

Heated to 100° C. it lost 4 atoms of water; and 0·420 gave 0·411 sulphate of zinc, or 49·082 per cent. oxide of zinc; atomic weight, 2052·3. 0·4225 gave 0·0715 water, and 0·334 carbonic acid, which gives the formula $C^{12} H^5 O^{11} + 4 ZO$. This salt, however, is then essentially altered, part of its malic acid being converted into fumaric acid, as will be shown in the sequel.

Acid Malate of Copper. $\overline{M}$ Cu O H² O + 2 aq
$\overline{M}$ Cu O H² O (100° C.).

Prepared by dissolving hydrated oxide of copper in malic acid, and evaporating at a temperature of 30° to 40° C., as a small blue crystalline body. 0·690 of this salt gave 0·149 oxide, or 21·521 per cent. of oxide of copper; atomic weight, 2302·17.

The composition calculated from this, is as follows:—

		Calculated.	Found.
1 atom Malic acid	1461·39	63·69	
1 ... Oxide of copper	495·70	21·60	21·821
3 atoms Water	337·44	14·71	
	2294·53		

Dried at 100° it loses 2 atoms of water, and its atomic weight becomes 2069·57.

Malate of Silver. $\overline{M}$ 2 Ag O.

0·2877 salt gave 0·1777 silver, or 66·339 per cent. of oxide of silver. Hence

		Calculated.	Found.
1 eq. Malic acid. . .	1461·39	33·48	33·66
2 ... Oxide of silver	2903·21	66·52	66·33
	4361·60		

Malate of Barytes. $\overline{M}$ 2 Ba O + 2 aq
$\overline{M}$ 2 Ba O (100° C.).

A solution of malic acid is saturated with barytes water, and evaporated at a very moderate temperature. The salt

found crystallizes. At 30° C. it loses one atom of water; at 100° C. it becomes quite anhydrous.

Of the salt, 0·5575, dried at the ordinary temperature, gave 0·452 sulphate of baryta, or 53·207 per cent. of barytes; which makes the atomic weight 3956·8, the calculated one being 3600·10. The salt dried by 30° C. gave 54·426 per cent. barytes. That dried at 100° C. is composed of 1 atom of malic acid and 2 atoms of barytes.

Malate of Strontian. $\overline{M}$ 2 St O + 3 aq
$\overline{M}$ 2 St O + 2 aq (100° C.).

Fumarate of Oxide of Æthyl.

This æther is formed whenever malic acid is brought into contact with muriatic æther. Malic acid also, when long mixed with absolute alcohol, or with strong fuming hydrochloric acid, is converted into fumaric acid.

This combination, or fumaric æther, is heavier than water, and has a grateful odour, like that of fruit. It is slightly soluble in water, and is therefore better separated from muriatic æther, when mixed with the latter by distillation, than by means of water. By potash, fumaric æther is converted into alcohol and the fumarate of potash. Kept long in contact with ammonia, it is converted into fumaramide. Purified by being distilled over chloride of calcium, 0·3315 æther gave 0·208 water, and 0·669 carbonic acid. Hence

		Calculated.	Found.
8 atoms Carbon . .	611·48	56·29	55·803
6 ... Hydrogen	74·87	6·89	6·97
4 ... Oxygen . .	400·00	36·82	37·22
	1086·35		

Fumaramide. $C^4 H O^2, N H^2$.

This amide is obtained when fumaric æther is left a long time in contact with an excess of caustic ammonia. Its formation is quite analogous to that of oxamide and the other compounds of amide. In cold water and absolute alcohol it is quite insoluble. It is soluble in boiling water, and again precipitates as the water cools. Left long in contact with water, it is completely converted into fumarate of ammonia. Ammonia is disengaged by the fixed alkalies, and a fumarate formed. By distillation it is decomposed into ammonia; a white body, probably maleinic acid, and a residue of charcoal are left.

0·426 of fumaramide gave 0·1335 water, and 0·2780 car-

bonic acid. By a qualitative determination of the nitrogen, the latter was found to be to the carbonic acid in volume as 1 to 4.

Carbon	42·37	
Hydrogen . . .	5·33	
Nitrogen . . .	24·53	
Oxygen	27·77	
		100·00

This gives—

			Calculated.
4 atoms	Carbon	305·74	42·46
3 ...	Hydrogen . . .	37·44	5·19
1 ...	Nitrogen . . .	177·04	24·59
2 ...	Oxygen	200·00	27·76
		720·22	100·00

Malates at High Temperatures.

If malates of the earths or alkalies are kept for some time at a temperature varying from 250° to 300° C., they are changed into fumarates, water being the only other product. The changes produced are best observed in the following manner. The fumarate produced is dissolved in as small a quantity of boiling water as possible, and a small excess of nitric acid is added to it. The fumaric acid crystallizes from the solution in its peculiar form, possessing all the properties ascribed to it by Pelouze. I have prepared its silver salt to identify it with certainty. 0·2726 acid gave 0·0885 water, and 0·4115 carbonic acid. This gives the following formula for its composition:—

			Calculated.	Found.
4 atoms	Carbon. . .	305·74	41·84	41·73
2 ...	Hydrogen	24·95	3·41	3·60
4 ...	Oxygen . .	400·00	54·75	54·683
		730·69		

0·3735 of the silver salt gave 0·3205 chloride of silver, or 69·422 oxide of silver; and 0·4270 gave 0·029 water, and 0·224 carbonic acid.

		Calculated.	Found.
4 atoms Carbon	305·74	14·77	14·50
1 atom Hydrogen . . .	12·48	0·60	0·75
3 atoms Oxygen	300·00	14·49	15·31
1 atom Oxide of silver	1451·6	70·14	69·42
	2069·82		

This remarkable change of malic acid salts into those of fumaric acid, appears to me to bear a strong analogy to the formation of the pyro- and metaphosphates, but this is as yet not sufficiently proved by experiment. I have kept a saturated solution of fumaric acid at a boiling temperature for several days without the slightest change in it. And I have also kept a like solution, in a tube hermetically sealed, for a considerable time at a temperature of 250°, under a pressure therefore of nearly 15 atmospheres, without its being altered in any of its properties. Hence fumaric acid does not appear to be reconvertible into malic acid.

XLVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 175.]

Jan. 20, 1841. A PAPER was first read, "On the Teeth of Species of the Genus *Labyrinthodon* (*Mastodonsaurus Salamandroides*, and *Phytosaurus* (?) of Jäger) from the German Keuper and the Sandstone of Warwick and Leamington," by Richard Owen, Esq., F.G.S., F.R.S.

The Warwick sandstone having been considered by some geologists to be the equivalent of the Keuper*, and by others of the Bunter Sandstein†, and as its true position remains to be determined, Mr. Owen, in the preliminary remarks to his memoir, points out the assistance which the discovery of reptilian remains in the Warwick sandstone of the same generic characters as those of fossils obtained in the Keuper of Germany, may afford in determining the question.

Before he proceeds to describe the fossils forming the immediate object of his paper, Mr. Owen shows that the genus *Phytosaurus* was established on the casts of the sockets of the teeth of *Mastodonsaurus*; and that the latter generic appellation ought not to be retained, because it recalls unavoidably the idea of the mammalian genus *Mastodon*, or else a mammilloid form of the tooth, whereas all the teeth of the genus so designated are originally and, for the greater number, permanently of a cuspidate and not of a mammilloid form; and because the second element of the word, *saurus*, indicates a false affinity, the remains belonging, not to the Saurian, but to the Batrachian order of Reptiles. For these reasons, and believing that he has discovered the true and peculiarly distinctive dental characters of the fossil, he proposes to designate the genus by the term *Labyrinthodon*.

The only portions of the Batrachian found in the Keuper of Ger-

* See Proceedings, vol. ii. p. 453. [or Phil. Mag., Third Series, vol. xi. p. 106.]

† Ibid. vol. ii. p. 565. [or Phil. Mag., Third Series, vol. xi. p. 320.]

many, which have hitherto been described, consist of teeth, a fragment of the skull, and a few broken vertebræ; and in the Warwick sandstone of teeth only. In this memoir, therefore, Mr. Owen confines his attention to a comparison of the dental structure of the Continental and English remains. The teeth of the *Labyrinthodon Jaegeri* (*Mastodonsaurus Jaegeri*, Meyer) of the Keuper are of a simple, conical form, with numerous fine longitudinal striations; and the teeth transmitted to Mr. Owen from the Warwick sandstone by Dr. Lloyd, bear a very close resemblance to them. Their external characters not being sufficient to establish either specific or generic identity, Mr. Owen had sections prepared for microscopic examination of portions of teeth of the *Labyrinthodon Jaegeri* forwarded to him by Prof. Jäger, and of the English reptile; and though, from his previous examination of the intimate texture of the teeth of the Plesiosaur, Megalosaur, as well as of the Crocodile, Monitor, and most recent Lacertians, he did not hope to detect such modifications of structure as would obviously mark specific or even generic identity, yet the slices exhibited such decided characters, and those of the German fossils agreed so intimately with the sections obtained from the Warwick specimen, that Mr. Owen was enabled not merely to separate these fossils from all known reptilian animals, but to establish a generic community of character in the Keuper and sandstone remains. It was not, however, until he had caused sections to be made in various directions, and had studied them attentively in comparison with the teeth of true Saurians, Batrachians, and other animals, that he was enabled to comprehend the principle of the singular cerebriform convolutions which pervade the dental structure of this remarkable reptile. The base of the tooth of the *Ichthyosaurus* approaches most nearly in character to the peculiarities of nearly the entire tooth of the *Labyrinthodon*. It is impossible to convey clearly without illustrations the structure alluded to. It may, however, be stated, that in the fang of the tooth of the *Ichthyosaurus* vertical folds of the external layer of cement (the enamel ceasing at the base of the crown) are inflected inwards, at pretty regular distances around the circumference of the tooth, towards the centre to a distance about equal the breadth of the interspaces of the inflected folds; the interspaces being occupied by corresponding processes of the dentine, which radiate from the central mass of that substance. The thickness of this interblended cement and dentine, surrounding the pulp-cavity, is about one-eighth of the diameter of the tooth.

The plan and principle of the structure of the tooth of the *Labyrinthodon* are the same as those of the tooth of the *Ichthyosaurus*, but they are carried out to the highest degree of complication. The converging vertical folds of the external cement are continued close to the centre of the tooth, and, instead of being straight, simple lamellæ, they present a series of irregular folds, increasing in complexity as they proceed inwards, and resembling the labyrinthic anfractuosities of the surface of the brain; each converging fold is slightly dilated at its termination close to the pulp-cavity. The

ordinary laws of dental structure are, however, strictly adhered to, and every space intercepted by a convolution of the folds of the cement is occupied by corresponding processes of the dentine. These characters were presented by a transverse section of a fragment of a tooth of the *Labyrinthodon Jaegeri* from the German Keuper, which included about the middle third part of a tooth, and Mr. Owen considers that the entire length of the tooth might be $3\frac{1}{2}$ inches, and the breadth at the basis $1\frac{1}{2}$ inch.

The external longitudinal grooves, which correspond to the inflected folds of the cement, extend upwards from the base of the tooth to about three-fourths of its height, decreasing in number as the tooth diminishes in thickness, and disappearing about half an inch from the summit of the tooth. Each fold of cement penetrates less deeply as the groove approaches its termination; and Mr. Owen conceives that the structure of the upper part of the tooth may be more simple than that of the lower, but he has not yet been able to extend his investigations to it.

The dentine consists of a slender, central, conical column or "modiolus," hollow for a certain distance from its base, and radiating outwards from its circumference a series of vertical plates, which divide into two, once or twice, before they terminate at the periphery of the tooth. Each of these diverging and dichotomizing vertical plates gives off throughout its course narrower vertical plates, which stand at nearly right angles to the main plate, in relation to which they are generally opposite, but sometimes alternate. Many of the secondary plates, which are given off near the centre of the tooth, also divide into two before they terminate. They partake of all the undulations which characterize the inflected folds of the cement.

The central pulp-cavity is reduced to a line, about the upper third of the tooth; but fissures radiate from it, corresponding in number with the radiating plates of the dentine. One of these fissures is continued along the middle of each plate, dividing where it divides, and penetrating each bifurcation and process; the main fissures extend to within a line or half a line of the periphery of the tooth; the terminations of these, as well as the fissures of the lateral processes, suddenly dilating into subcircular, oval, or pyriform spaces. All these spaces constitute centres of radiation of the fine calcigerous tubes, which, with their uniting clear substance, constitute the dentine. The number of these calcigerous tubes, which are the centres of minor ramifications, defies all calculations. Their diameter is the $\frac{1}{7000}$th of a line, with interspaces equal to seven diameters of their cavities.

Mr. Owen then compares the structure of the section of a tooth procured in the sandstone of Coton-End Quarry, and lent to him by Dr. Lloyd of Leamington. The tooth nearly resembles in size and form the smaller teeth of *Labyrinthodon* figured by Prof. Jäger. All the peculiarities of the labyrinthic structure of the Keuper tooth are so clearly preserved in this specimen, that the differences are merely of a specific nature.

At the upper part of the tooth a thin layer of enamel*, besides a coating of cement, is inflected at each groove towards the centre of the dentine; but about the middle of the tooth the enamel disappears, and the convolutions consist of interblended layers of cement and dentine. Thus, on the supposition that the tooth of the *Labyrinthodon* of the German Keuper be capped with enamel, its extent must be less than in the tooth of the Warwick sandstone.

The inflected folds are continued for a greater relative distance before the lateral inflections commence than in the German species, and the anfractuosities are fewer in number, and some of the folds are reflected backwards from near the central pulp-cavity for a short distance before they terminate.

The modifications of the complex diverging plates of the dentine hardly exceed those of a specific character, and the dentine itself is composed of calcigerous tubes of the same relative size and disposition as in the *Labyrinthodon Jaegeri.*

In a section taken from the middle of a smaller and relatively broader and shorter conical tooth from the Warwick sandstone, Mr. Owen found that the anfractuosities were more complicated, with numerous secondary and tertiary foldings, and the external layer of cement was relatively thicker than in the *Lab. Jaegeri.*

The generic identity of the Reptiles, indicated by the teeth from the Warwick sandstones, with the *Mastodonsaurus* of the German Keuper, Mr. Owen believes to be fully established by the concordance of their peculiar dental structure above described. And in conclusion, he says, if, on the one hand, geology has in this instance really derived any essential aid from minute anatomy, on the other hand, in no instance has the comparative anatomist been more indebted to geology than for the fossils which have revealed the most singular and complicated modification of dental structure hitherto known; and of which not the slightest conception could have been gained from an investigation, however close and extensive, of the teeth of existing animals.

A paper was next read, entitled "Observations relative to the Elevation of Land on the shores of Waterford Haven during the Human Period, and on the Geological Structure of the District," by Thomas Austin, Esq.

The shore on the west side of Waterford Haven, from the rock of Passage to Woodstown, a distance of three miles, presents an almost uninterrupted cliff of clay and gravel, composed chiefly, if not wholly, of detritus of old red sandstone, and enclosing a bed from one to four feet thick of *Cardium edule,* with other marine testacea of existing species, and a few land-shells. This bed of shells is not confined exclusively to the coast, but it extends inwards to the distance of eight miles, distinct traces of it occurring between Waterford and Tramore, and at several intermediate points. In the alluvial valley of Woodstown, close to Newtown Head, the shells rest on an ancient

* Mr. Owen has subsequently ascertained that this is not true enamel, but a layer of firm dentine, separated from the rest by a thin stratum of fine calcigerous cells.

peat bed, raised but a few inches above the sea-level. On the eastern side of Waterford Haven beds of similar shells occur at the same level; also in the cliff north of Bluff Head, at the height of eight feet. The greatest elevation at which the shelly beds have been observed by Mr. Austin in the county of Waterford, is forty feet.

Immediately north of Newtown Head, at the point where a gradual rise takes place in the cliff, the greater part of a human skeleton was found resting on its back, five feet three inches below the surface, and about the same distance above high-water level, in the centre of the shelly bed. The *Cardium edule* was as numerous in and around the skeleton as in other portions of the bed, many of them being lodged in the cavity of the skull. Mr. Austin carefully examined the conditions under which the skeleton was found, and he is convinced that the ground had never been disturbed for sepulture, the continuity of the shelly bed being unbroken where the skeleton occurred, and no specimens of the *Cardium edule* being dispersed at random through the incumbent loam. He is therefore of opinion, that the body was washed into the estuary during the period when the shelly bed was accumulated; that it was arrested at the point where it has been found by the rise in the level of the bed; and that consequently an elevation of the country has taken place since the commencement of the human period.

From an extended examination Mr. Austin is convinced, that the estuary now limited to Waterford Haven formerly covered a much larger area, as proved, in part, by the patches of shells noticed above; and that the change of relative level has been slow and uniform, producing no local disturbances; and he is further of opinion, that the operation may be still in progress.

Mr. Austin then gives a general description of the geological structure of the two shores of Waterford Haven and of the adjacent districts. The formations consist of mountain-limestone, old red sandstone, schistose strata, considered to be of the age of the Silurian system on account of the fossils found near Duncannon Fort and Newtown Head, and trap rocks.

The mountain-limestone constitutes Hook Point, the southernmost headland of the Wexford side of the Haven. It is succeeded to the north, conformably, by a red or yellow sandstone, containing obscure vegetable remains, also thin seams and nodules of anthracite, likewise some small masses of black copper ore. These beds are assigned by the author to the upper part of the old red sandstone. They are succeeded in regular descending order by various marls, sandstones and conglomerates, composing the mass of the formation, and estimated to be 1600 feet thick. A series of contorted and tilted slaty beds are then presented; but at Broom Hill the conglomerates of the old red sandstone reappear with the same dip towards the south. Immediately north of this promontory the slates recommence, and are displayed in unconformable juxtaposition with the old red sandstone, the latter dipping southwards, and the former at a higher angle northwards. From Broom Head to Arthurstown the slates constitute the whole line of coast, except at Duncannon Fort. The strata

are, for the greater part, variously contorted; but near Arthurstown they dip 70° to the north, and are overlaid by beds of old red sandstone, which also dip to the north, but at an angle of only 30°. At Duncannon Fort an impure limestone occurs, containing Trilobites, corals and testacea, and considered by Mr. Austin to be analogous to species found in the Silurian system.

On the opposite or Waterford side of the Haven the old red sandstone occurs at Creden Hill and Knockavelish Head, eminences corresponding to Broom Hill; a small patch of it is displayed a little to the northwards, inserted unconformably in the slate series; and it forms the rock of Passage, a prolongation of the old red sandstone near Arthurstown. Between Knockavelish Head and Passage the slate series prevails, except near Newtown Head, where trap-rocks are exposed. A little to the north of that headland are some highly inclined fossiliferous strata, corresponding in position to the beds near Duncannon Fort on the opposite side of the Haven; they are visible only at ebb-tide.

The trap-rocks constitute the point on which stands Duncannon Fort; Newtown Head is also formed of trap; and Mr. Austin is of opinion that the same mass strikes westwards to Tramore and thence to Great Newtown Head, where it is lost in the St. George's Channel. Along this line, wherever the trap comes to the surface, the slates are tilted.

With respect to the numerous contortions exhibited in the schistose rocks, Mr. Austin ascribes their existence to lateral pressure, which he says must have been excessive; and he is of opinion that a considerable portion of the upper part of these contorted beds has been removed by denudation.

A paper by C. Lyell, Esq., F.G.S., was afterwards read, "On the Freshwater Fossil Fishes of Mundesley, as determined by M. Agassiz."

In a memoir on the boulder formation and associated freshwater deposits of Eastern Norfolk*, Mr. Lyell stated, on the authority of Mr. Yarrell and the Rev. L. Jenyns, that the scales and teeth of fishes which had been then procured in the fluviatile beds of Mundesley belonged to the *Esox lucius*, to a trout or an undeterminable species of *Salmo*, to a carp, probably the *Cyprinus carpio*, and to a distinct species of *Perca*.

This collection, with some additions recently sent to the author by Mr. Wigham, was examined by M. Agassiz during his late visit to England. The decision of Mr. Jenyns with respect to the distinctness of the perch, M. Agassiz fully confirmed; but he was of opinion that the pike differs from the *Esox lucius*, and that the supposed carp is a species of *Leuciscus*; and that the trout is not truly a trout, although one of the same great family.

From this examination, therefore, Mr. Lyell says it is apparent that these remains belong to species not identical with any European

* See Proceedings, *antè*, p. 171. [The paper here referred to appeared in Phil. Mag., Third Series, vol. xvi. p. 345.]

freshwater fishes hitherto described; but that they nevertheless belong to an ichthyological fauna, more modern and more nearly resembling the recent than any other with which M. Agassiz is acquainted in a fossil state.

Similar remains have been found by Mr. Lyell at Runton, near Cromer, but both there and at Mundesley the associated testacea all belong to living freshwater species; even the *Paludina minuta* (Strickland), which Mr. Morris has pointed out to the author to be identical with the *P. marginata* of Michaud, a living French species. It is a question therefore, the author states, whether these unknown fishes may not still inhabit the rivers and lakes of the more northern parts of Europe or America, especially as M. Agassiz is at present unacquainted with the freshwater fishes of Norway, Sweden, Spitzbergen, Iceland, Greenland, Labrador and Canada, and even of the northernmost parts of Scotland and the Shetland Islands; and in conclusion Mr. Lyell says, it seems natural to look northward for types analogous to the Mundesley fishes, because the beds in which they occur were deposited contemporaneously with the drift accumulated by the agency of floating ice.

Feb. 3.—A paper was read, "On the Geological Structure of the Wealden District, and of the Bas Boulonnais," by William Hopkins, Esq., F.G.S.

This paper is divided into two parts. In the first the author describes the phænomena of elevation presented in the two districts comprised respectively within the boundary of the great Chalk escarpment of the south-eastern part of England, and an exactly similar escarpment forming the inland boundary of the Bas Boulonnais. The former is well known as extending from the coast at Folkstone, by Seven Oaks, Godstone, Farnham, Petersfield, &c., round to the coast again at Beachy Head. On the opposite side of the channel, the escarpment, commencing at Wisant on the north, forms nearly a semicircle, of which Boulogne is not far from the centre. If we conceive the northern Weald escarpment continued from Folkstone to Wisant, and the southern one from Beachy Head to the southern extremity of the Bas Boulonnais, it will be seen that the whole tract comprised within the Chalk would be a regular oval, except that its axis instead of being straight is *curved*, so as to incline towards the S.E. in its eastern portion. These two districts are thus connected by relative position not less than by a community of geological character.

In the second part of his paper the author compares the laws of the existing phænomena in these districts with the results given by his 'Theory of Elevation,' published in the Transactions of the Cambridge Philosophical Society (Vol. VI. Part I.).*

I. The lines of elevation in the Wealden district are partly marked by an anticlinal arrangement of the beds, and partly by strong flexures, forming *one-sided saddles*. The latter have been termed by the

[* Mr. Hopkins gave a view of this subject in Phil. Mag., Third Series, vol. viii. p. 227.—Edit.]

author *lines of flexure*. The central portion of the district is first described. The following lines of elevation are found in it.

1. *Hastings Line*.—This line runs to the north-east of Hastings towards Battle. It has been mentioned by Dr. Fitton and other geologists. The author had not had time to examine it himself.

2. *Brightling Line*.—This is strongly anticlinal, and runs along the high ridge of Brightling Down as far as Heathfield Park, where its distinct features are lost. The author has not ascertained whether it is a continuation of the Hastings line.

3. *Wadhurst Line*.—This line runs by Wadhurst and Hawkshurst, to the south-west of which place it is lost. It also ranges westerly along the ridge between Wadhurst and Mark Cross.

4. *Crowborough Line*.—Crowborough Beacon stands on what must be regarded as the great central ridge of the district. The anticlinal line runs near the Beacon and is continued westerly to the north of Balcombe. No traces of it however are distinguishable beyond Horsham.

5. *Cuckfield Line*.—This line extends parallel to that last described, and immediately to the north of Cuckfield. It is not to be traced far to the west of that place. To the east it is continued across the Brighton railway, where it was very distinctly exhibited in the new cuttings.

6. *Frant Line*.—At Lamberhurst this line is distinctly marked. It proceeds westward along Frant Hill, where its evidence, however, is not very distinct. It appears to be lost entirely not far to the west of Frant.

7. *Bidborough and Brenchley Line*.—Bidborough Hill is formed by a strong flexure of the beds by which the Hastings sand is brought up from beneath the Weald clay. Brenchley Hill is formed in the same manner, but presents a more distinct anticlinal arrangement. These hills are separated by a wide transverse valley of denudation, but there can be little doubt, it is conceived, that they belong to the same line of elevation. The dislocation is also continued westward, but with less distinctness, across the Medway.

All these lines preserve a remarkable parallelism with each other and with the *curved central axis* of the district.

The author also describes several transverse valleys of the central portion of the district, and states the evidence on which he believes them to have originated in transverse dislocations.

8. *Greenhurst Line*.—This line has been described by Mr. Martin, by whom it was first detected. It is distinctly marked from a point south-west of Pulborough, whence it runs not far from and parallel to the chalk escarpment, till it strikes into the chalk at Piecomb. Its continuation westward is not very distinct, but eastward it is strongly defined at Lewes. Several remarkable transverse valleys across the greensand ridge are also found in the south-western part of the district, and present evidence of having originated in transverse dislocations. Their directions are as nearly as possible perpendicular to that of the Greenhurst line.

9. *Line from Farnham to Seven Oaks*.—This line runs parallel to the chalk escarpment of the North Downs and near to it. It is a

line of flexure, with a great dip to the north, but without the corresponding dip to the south necessary to form an anticlinal arrangement, except in one or two localities. Towards the west it runs immediately at the foot of the Hog's Back with a dip, which, near its western extremity, amounts to 70 or 80 degrees. Near Guildford it passes by the foot of the hill on which Margaret's chapel stands. To the east it passes south of Dorking and Reigate to the summit of Tilburstow Hill. It is afterwards continued by Limpsfield to the east of Seven Oaks, as formerly described by Dr. Fitton. At some points between these last-mentioned places, the line assumes a distinct anticlinal character.

Transverse valleys exist in the greensand ridge of this part of the district as well as in that on the southern side. The author also alludes to what he conceives to be incipient valleys of this description, and states his reasons for believing them to be indications of transverse fractures. He conceives this opinion to be strongly corroborated by the existence of the perennial springs by which these valleys are characterized. Several are pointed out, especially in Leith Hill and the Seven Oaks ridge overlooking the valley of the Weald.

Transverse river-courses through the Chalk escarpment form one of the most striking features in the geology of this district. The analogy which they bear to the transverse valleys across the greensand ridges would seem to leave no doubt of their being referable to the same physical cause; and as there are in many instances direct evidence which renders the origin of these latter valleys in transverse fractures highly probable, the same conclusion appears almost equally probable with respect to the river-courses through the Chalk. In the evidence of dislocation which the Chalk itself affords, there is nothing, however, very conclusive; but it must be remembered, that the evidence of faults is always difficult to detect in a massive formation like the Chalk, possessing not more than two general divisions which admit of distinct identification.

The central chalk ridge of the Isle of Wight is traversed in like manner by three transverse valleys, two of which are river-courses. The author has pointed out some direct evidence in support of the conclusion, that the central one (that of the Medina) has originated in transverse dislocation.

Bas Boulonnais.—With respect to the structure of the Bas Boulonnais, it is only necessary here to state, that the author has recognised three parallel lines of dislocation commencing at the coast and running in a direction coinciding with that of the lines of elevation of the Weald, supposing them produced across the Channel according to the law which they follow on this side of it. The southernmost of these lines passes immediately to the north of Boulogne.

II. In the second part of this paper, previously to his comparing the observed phænomena with theoretical deductions, the author recapitulates some fundamental points of his theory. It is assumed, that an elevatory force has acted simultaneously at every point of the lower surface of the elevated mass in each district throughout which the phænomena of elevation are observed to follow the same law.

This force is not supposed to have been necessarily of uniform intensity throughout. If it has been greater in one portion of the district than in the rest of it, a corresponding modification will be produced in the directions of the lines of elevation, or a deviation from those positions in which they would have existed had the intensity of the force been uniform throughout. If the force has been uniform, the directions of the lines of dislocation and elevation will depend on the *form* of the boundary of the surface of the elevated area. If this be given, these directions must be investigated on mechanical principles; and if the force be supposed to have acted with greater intensity in any assigned portion of the district, the corresponding modification in the directions of the lines must be determined. This has been done by the author in some particular cases in the memoir above referred to in the Transactions of the Philosophical Society of Cambridge.

Any irregularity in the cohesive power of the elevated mass will have but little effect on the general directions of the lines of elevation; but if there be any considerable continuous portion of the district throughout which the elevated crust is *thinner*, and therefore lighter and weaker, the effect will manifestly be the same as if the crust had been of uniform thickness throughout, and had been acted on in this particular portion with a force of greater intensity. Consequently the modifications in the lines of elevation will be the same, whether they arise from a weaker crust or a greater intensity of force.

In the application of this theory, the boundary of the area under which the elevatory force has simultaneously acted must be determined as nearly as may be by the actual boundary of the disturbed district, throughout which we recognize a character of continuity in the phænomena of elevation. That portion of the district also in which the force may appear to have acted with greater intensity must be determined by the existing indications of greater elevation. Thus it is conceived, that a simultaneous effort of the elevatory force was made throughout the whole tract extending from the Bas Boulonnais on the east, beyond the Wiltshire Chalk on the west, and from the Vale of Pewsey and the valley of the Thames on the north, beyond the southern coast of this country on the south. The Wealden district, with the Bas Boulonnais, presents us also with a case, in which it is presumed, from its greater elevation, either that the disturbing force acted there with greater intensity, or that the elevated crust was there thinner, than in the other part generally of the district. Assuming such to have been the case, the author points out what would be the general directions of the lines of elevation throughout the Wealden and the Bas Boulonnais, and comparing them with the lines described in the first part of his paper, he shows the remarkable accordance which exists between the results of observation and of theory; an accordance which he considers as strongly confirmatory of his theory as applied to this district.

Hence the author concludes, that the fissures or dislocations in which he conceives all the observed lines of elevations (whether faults, anticlinal lines, or lines of flexure,) to have originated, must

have been produced by one simultaneous and momentary effort of the elevatory force. It is not, however, to be regarded as a necessary consequence of this conclusion, that the whole elevation of the district was thus produced at once; it might be in some degree produced by previous, and in a considerable degree by subsequent movements; but it would seem at least highly probable, that that general movement which produced the dislocations of the elevated mass, and impressed upon it its present distinctive characters, should have been the most energetic of those repeated movements to which the whole elevation has probably been due.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 245.]

Dec. 11, 1840. (*Continued.*)—Description of a Method of dividing one Circle, B, by copying from another, A, previously divided. By Lieut.-Col. Everest, Director of the Trigonometrical Survey of India.

In Col. Everest's 'Account of the Measurement of an Arc of the Meridian in India,' published in 1830, it was stated that, with a view to avoid the effects of the errors of catalogues and the periodical irregularities to which all stars are more or less liable, it had been resolved to substitute two astronomical instruments, each with a vertical circle of three feet and an azimuth circle of two feet diameter, for the zenith-sector of Ramsden, formerly used in determining the amplitudes by his predecessor and himself; the intention being, that all celestial amplitudes should be determined by observations taken simultaneously at both extremities of the arc. Accordingly, two new altitude and azimuth instruments were ordered in London and forwarded to India, where they arrived in 1832; but an opportunity of giving them a fair trial did not occur until 1837, when a small observatory was constructed at Kaliana, the northern limit of one of the sections of the great arc of the meridian. The instruments proved, on trial, to be of too feeble construction for the purpose for which they were intended, and Colonel Everest found that, in order to render them serviceable, it would be necessary to construct for each a pair of stronger columns, new outriggers and friction-rollers, a new axis for the azimuth motion, a new table for supporting the columns, and a new azimuth circle; in short, to construct two entirely new instruments, with the exception of the vertical circles. To any other than a professed artist, an operation of this kind, even under the most favourable circumstances, would prove an arduous and formidable undertaking; but, in a half-barbarous country like Upper India, where no artificer was to be found who had ever tried his hand at dividing, or even seen a cutting-tool, it could not be otherwise than extremely embarrassing. Before Col. Everest undertook the alterations, he had obtained, indeed, the assistance of a mathematical instrument-maker in the service of the Company, at Calcutta; but, owing to some disagreement, this person left him before the most difficult part of the operation, that of dividing the

new circles, was ready to be commenced, and he was thereby thrown entirely upon his own resources.

Colonel Everest resolved to construct the new tables and azimuth circles of cast-iron, which, though objectionable on account of its liability to rust, has the advantage of being lighter and less yielding than brass. The circles were inlaid with gold to receive the graduations; and at every tenth degree a circular disc of silver was let in, so that the large figures might stand prominent. Each was supplied with a strong edge-bar below, which put all yielding out of the question, and moreover was of great service in the process of graduation, by affording a support on which to affix the reading microscopes. With respect to the mode of dividing the circles, Col. Everest states that, being aware of the dilatoriness and difficulty of any method he had seen detailed, he devised a plan of his own, whereby the divisions of the original circles should be copied on the new. Without the aid of drawings, it is scarcely possible to impart an idea of the mechanical means which were employed for carrying this plan into effect. The two circles, the new one which was to be graduated, and the old one from which the graduation was to be copied, were placed the one directly over the other, and strongly secured by screws; and the principle of the method consists in keeping the circle to be graduated, which is placed below, steadily fixed on a substantial pillar of masonry, and making the circle to be copied from revolve above it, carrying with it the cutting-tool and clamp: and the apparatus being thus arranged, it is obviously only necessary to apply sufficient means of reading off the angular spaces passed over by the latter in its rotation, in order to transfer its graduation to the former. The mode of operation was assimilated to that which is followed in taking angles. A skilful person was appointed to each of the four microscopes; the different readings were made precisely equal to the arcs to be intercepted; and when the one mean reading subtracted from the other showed this value, the order was given to cut.

The author describes in minute detail the different parts of the operation, which appears to have been executed successfully, and he concludes his paper with an investigation of the mathematical relations of the method, and particularly the effect of eccentricity of the upper circle, and of any error in fixing the cutting-tool, whereby its point would be made to move obliquely to the radius.

Transits observed at Washington (United States), from January 1 to July 1, 1840; and Occultations observed at the same place, since June 1839. By J. Melville Gillies, Esq.

The transit is one of the 6-feet instruments made by Troughton for Mr. Hassler, in 1815, and mounted on substantial granite pillars. The usual methods were adopted for determining its errors of level and collimation, and the observations are free from all such. The deviation in azimuth was determined from the observed and true differences of right ascension of high and low stars, and registered in its proper column; but the proportional part due to each observation was not in any case applied. The observations

were registered by a chronometer regulated to sidereal time, and its rate determined by a mean of successive transits of the same star. The true sidereal time and right ascensions were taken from the list of moon-culminating stars, the list of 100 stars, and the list of stars liable to occultation given in the Nautical Almanac; and the remainder computed by means of the Royal Astronomical Society's constants. The moon's right ascension was determined by applying the mean error of the chronometer to the observed right ascension.

Places of Bremicker's Comet, as determined with the Equatoreal Telescope at Mr. Bishop's Observatory. By the Rev. W. R. Dawes.

Date.	Greenwich Mean Time.	Right Ascension.	N.P.D.	Declination.	Remarks.
1840.	h m s	h m s	° ′ ″	° ′ ″	
Nov. 14.	9 4 19	......	30 39 6	= +59 20 24	Rather doubtful.
	9 29 9	20 31 13	......	......	Ditto.
16.	9 55 53	20 46 37	31 13 11	= +58 46 49	Tolerably good.
19.	8 38 13	21 9 18	32 15 3	= +57 44 57	Good.
	9 37 59	21 9 33	32 16 11	= +57 43 49	Ditto.
	9 50 2	21 9 39	32 16 39	= +57 43 21	Ditto.
21.	9 9 39	21 24 55	33 7 2	= +56 52 58	Very good.
24.	8 51 1	21 48 4	34 37 47	= +55 22 13	Excellent.
26.	6 54 40	22 2 43	35 45 10	= +54 14 50	Very good.
27.	5 43 15	22 10 3	36 21 10	= +53 38 50	Comet excessively faint, seen through smoke and fog. Doubtful observation *on meridian.*
Dec. 2.	7 31 58	22 46 39	40 0 11	= +49 59 49	Very good.
3.	6 33 59	22 53 14	40 46 8	= +49 13 52	Excellent.

LONDON ELECTRICAL SOCIETY.

Sept. 21, 1841.—The papers read at this meeting were,—1st, A letter from Mr. T. Pine to the Secretary, "On the Ripening or Maturing, and Decline of Vegetation." Third communication.

2nd. Notice of "The Effects of a Lightning Flash." By Mr. William Trackelton.

3rd. Notice of "A Test for Nitric Acid in the Sulphuric Acid employed in exciting Voltaic Batteries." By the Hon. Secretary.

Dissolve some sulphate of indigo in distilled water, and add some of the solution to the acid; if, on the application of heat for a few minutes, the blue colour disappears, nitric acid is present. This test was shown to the Secretary by Dr. Leeson, in the laboratory of St. Thomas's Hospital, in order that it might be communicated to the Society for the benefit of those who find their zinc consumed, and are not aware of this as one of the causes.

4th. Notice "On the Relative Powers of certain Diaphragm Voltaic Combinations," and on "A new Form for the negative Element of Voltaic Arrangements." By the Hon. Secretary.

The table of galvanometric deflections is suspended in the laboratory of the London Institution. Professor Grove furnished Mr. Walker with a copy for the Society; it contains the deflections for twenty-four arrangements. The negative element, also a suggestion of Mr. Grove's, is platinized silver gauze for Smee's battery; he has found the advantage from using cuprized copper, and has only delayed adopting the silver from the difficulty of obtaining such an article. The Secretary recommended platinized, plated-copper gauze.

5th. "On certain Phænomena connected with the Spark from a secondary Coil." By J. P. Gassiot, Esq., F.R.S., M.E.S., &c.

The wires of the coil were placed side by side, sometimes parallel, at other times at a considerable angle. In all cases the spark appeared at the cathode. With a coil containing twelve miles of wire, the secondary spark, in the flame of a spirit-lamp, was three-fourths of an inch long.

6th. "An Account of a Method of Electro-gilding and Electro-plating at the expense of a Gold or a Silver Anode." By Charles V. Walker, Esq., Hon. Sec.

This object is usually effected at the expense of a gold or silver solution, forming one of the exciting liquids in a single cell; and its strength is maintained by adding from time to time fresh supplies of the salt. It is obviously not the wisest principle to employ the salt if the metal itself can be used; but as the former plan has been so generally adopted, Mr. Walker suspected some physical difficulty might exist in the other mode. To satisfy himself, he went through a series of experiments, the results of which were, that from cyanide of silver, when electrolysed with a silver anode, an anion will be released that will combine with the anode, and that the like result attends the electrolysis of the cyanide of gold. The author employed a silver plate, which was considerably consumed; and a gold wire, which was also much acted upon. He recommends this as a surer, a more simple, and a much more œconomical method; and conceives that by this means the expense of each operation is reduced to a minimum. The communication extended to some length, and contained a description of a "Regulating Apparatus," to be employed when rich deposits are required, and instructions as to the management and preparation of the apparatus, &c.

Mr. Weekes's Monthly Register was then placed on the table, and the Society adjourned.

CHEMICAL SOCIETY OF LONDON.

[Continued from vol. xviii. p. 520.]

May 11, 1841.—"On a Simple and Cheap Method of preparing Hydrochloric Acid absolutely pure, and of any required strength," by Wm. Gregory, M.D. Professor, &c., King's College, Aberdeen.

Much difficulty is experienced in procuring pure and concentrated hydrochloric acid for chemical purposes, the common commercial acid containing various impurities, particularly sulphuric and sulphu-

rous acids, free chlorine, chloride of iron and sulphate of soda; these arise from impurities contained in the materials employed; the chlorine from the action of nitric or nitrous acid (often present in oil of vitriol) on the hydrochloric acid; sulphurous acid from organic particles in the common salt employed; and chloride of iron from the presence of that metal also in the salt. Pure and clean materials are therefore the first requisite for a pure acid. Dr. Gregory finds, that if to one equivalent of salt two equivalents of sulphuric acid diluted with a certain quantity of water be used, instead of one equivalent as usually prescribed, the whole of the hydrochloric acid may be expelled without a trace of sulphuric acid passing over even into the first condensing bottle, and that two-thirds of the hydrochloric acid distil over before water is volatilized; on this observation the following process is founded.

Into a common Florence flask are introduced 4 ounces of the purest patent salt, and 5 fluid ounces of sulphuric acid of specific gravity 1·600; a gentle heat is applied, and the gas which is then generated, is conducted by a bent glass tube into a four-ounce phial containing 2 ounces of distilled water surrounded with snow or ice-cold water. No safety tube is required, as the tube is made to dip only about one-eighth of an inch into the water, so that should any absorption take place the rise of a little water in the tube exposes the extremity of it, so as to admit the air; or, for greater security, a small bulb may be blown on the descending limb of the tube. The gas is absorbed as fast as it comes over, and for the first hour and one quarter the heat hardly requires to be increased; if the temperature of the surrounding water has been kept so low as 50°, the 2 ounces of distilled water will have increased in volume to 3 ounces of colourless hydrochloric acid, fuming strongly, and having a specific gravity of 1·20 to 1·21, the gas passing over so dry that no part of the tube becomes warm. This portion being removed, its place is supplied by 2 ounces more of distilled water, and the heat gradually increased and continued for an hour longer; by that time all the hydrochloric acid is expelled, with some water, and the 2 ounces of water have become 3 ounces of hydrochloric acid of specific gravity 1·10. Both portions are absolutely pure. If 3 ounces of water are used in the first instance, 4·5 fluid ounces of acid of specific gravity 1·165 are obtained; and then replacing the acid by 2 ounces of water, 3·5 ounces more of specific gravity 1·065. If 5 ounces of water are used at once for condensing the acid and kept till the distillation is complete, 7·5 fluid ounces of specific gravity 1·155 are obtained. Dr. Clark finds sulphuric acid of a specific gravity of 1·65 to answer still better than acid of 1·60.

Dr. Clark then exhibited to the Society his method of ascertaining, quantitatively, the comparative hardness of water by means of the common test of tincture of soap, illustrated by experimental evidence, to prove the accuracy of which it was susceptible, and the facility of its application. Dr. Clark hoped at a future meeting to lay before the Society more mature details of the method exhibited.

May 18.—An extract of a letter from Mr. Maugham was read, "On the Mode of treating Copper Ores, and the Ores of other Metals combined with Sulphur, so as to ascertain the quantity of Sulphur in such Ores, and also the quantity of Copper in the native Sulphuret."

A quantity of the powdered ore, sulphuret of copper, about 50 or 100 grains, is placed in a porcelain tube traversing a small furnace, and made red-hot; after remaining for 5 or 6 minutes a portion of the sulphur will be expelled; a stream of oxygen gas is then passed over it, the remaining sulphur is then rapidly given off as sulphurous and sulphuric acids, and the copper thoroughly oxidized. By heating the ore when first introduced into the tube it becomes slightly adherent, which prevents any of it from being blown away by the oxygen gas. The contents of the tube are then removed into an assay crucible, with the addition of black flax and a little charcoal; the whole covered with dry carbonate of soda or borax, and submitted to a yellow heat, when a button of copper is obtained. Mr. Maugham finds that arsenic and other volatile metals that may happen to be present, are oxidated and expelled by the heat; but should tin be present it will be found with the reduced copper, and must be removed in the usual way. The process is known to be complete when no more vapours are seen to issue from the tube, or when the odour of sulphurous acid is no longer perceptible. It is, however, to be observed, that white vapours will be seen even after the process is complete, owing to a portion of sulphuric acid condensed in the tube returning to the hot part. An assay of this kind takes about twenty minutes to execute.

When the wet analysis is desirable, we have only to proceed as before in the tube part of the process, and to dissolve the residue in the proper acids.

Mr. Maugham speaks favourably of the use of chlorate of potash, added to hydrochloric acid, for dissolving certain ores where nitromuriatic acid is generally employed, and afterwards expelling the excess of chlorine by heat; the known inconveniences of nitric acid in certain cases are thus avoided.

The quantity of sulphur contained in the ore is ascertained by elongating the tube traversing the furnace, so that it may dip into a vessel containing water saturated with chlorine, by which means the sulphurous acid is converted into sulphuric acid, and the quantity of sulphur found from the precipitate with chloride of barium.

A paper was read "On the Atomic Weight of Carbon," by Professors Redtenbacher of Prague and Liebig of Giessen*.

June 1.—Extract of a letter from Col. Yorke "On a Specimen of Artificial Arragonite."

"This substance was taken from the interior of a copper boiler which was used to supply hot water for household purposes at Port Eliot, Lord St. Germaine's seat in Cornwall. The substance is about $\frac{4}{10}$ths of an inch thick, and by its non-conducting power it caused,

* This paper is given entire in our last Number, p. 210.

as I understood, the destruction of the boiler. On the surface which was next to the copper it is coated by dioxide of copper, and the mass appears made up of an aggregation of prismatic crystals, whose axes are perpendicular to the surface on which the incrustation formed: under a microscope these crystals appear to be six-sided prisms. I compared, under a polarizing microscope, portions of the powder of Iceland spar, and of arragonite from Bilin, with the powder of the specimen; the latter agreed very closely in appearance with that of arragonite.

"Among the powder of the specimen were seen some very acute double six-sided pyramids; these with little doubt are similar to those formed by G. Rose* by evaporating solutions of carbonate of lime at a boiling heat, and described by him as resembling some sapphire crystals.

"On chemical examination it was evident that the specimen consisted chiefly of carbonate of lime; water, however, dissolved from it a small quantity of sulphate of lime.

"The following is the result of an analysis made on 10 grains, but which does not pretend to minute accuracy:—

Matter insoluble in muriatic acid, silica, with oxides of iron and copper.	1·3
Sulphate of lime	1·8
Carbonate of lime	93·7
Carbonate of magnesia	3·2
	100

"Deprived of its coating of dioxide of copper, three trials were made of the specific gravity of its powder; the sulphate of lime being previously washed off with hot water.

"The two first trials were made by weighing about 80 grains of the powder in a small spherical-stoppered phial (whose contents in distilled water at 62° was previously determined), and then when filled up with water, the third trial was made in the manner described by Rose. The specific gravity being thus determined, the powder was in each case dried, and slightly ignited, (by which operation arragonite, as is known, is converted into calcareous spar) and the specific gravity again taken. The results were as follows:—

	Spec. Grav. before ignition.	Spec. Grav. after ignition.
1st trial	2·842	2·708
2nd	2·828	2·701
3rd	2·878	2·681 †
Mean	2·849	2·696

The specific gravity of arragonite crystals from Bilin is 2·946.

"The highest specific gravity which Rose obtained of arragonite,

[* G. Rose's paper, here referred to, on the formation of calc-spar and arragonite, will be found in Phil. Mag., Third Series, vol. xii. p. 465.—Ed.]

† The loss by ignition on 43·8 grains was = ·08 grain.

formed by evaporating solutions of carbonate of lime, he states was =2·836.

"Specific gravity of Iceland spar is 2·72. I should suppose then that there can be little doubt but that the specimen affords an example of the formation of arragonite, and a verification of G. Rose's experiments.

"I have since made two attempts at producing arragonite by Rose's method of precipitation, but cannot boast of my success. The following is a note of the best experiment. A solution of 300 grains of chloride of calcium, in 4 ounces of water at 212°, was mixed rapidly with a solution of 330 grains of carbonate of ammonia in 8 ounces of water at 180°. The mixed liquor was not alkaline.

"The precipitate under the microscope consisted chiefly of radiating spicular crystals, extremely minute, with occasional rhombohedrons. The precipitate being washed, the specific gravity taken before drying came out = 2·751, after drying it was below 2·7. During the washing a slight crackling noise was heard, and I cannot help thinking the precipitate may have been thrown down as arragonite, but changed into calc-spar during the washing and drying."

Professor Kuhlman of Lille presented specimens of Chalk hardened by his process for the Silicification of Limestones, which consists of immersing them in a solution of silicate of potash, exposing to air for several days, and afterwards washing. Although the chalk did not contain more than three or four per cent. of silica, it was capable of scratching many cements and marbles. In a similar manner he could harden carbonate of lead and plaster of Paris. He finds alkaline salts in all the limestones containing silica, which are hydraulic, and believes that they originally resembled ordinary chalk in purity, but have been partially silicified by infiltration of water containing an alkaline silicate in solution, or by a natural process analogous to his artificial one.

Extract of a letter from Dr. R. F. Marchand of Berlin, "On the Atomic Weight of Carbon."

"I take this opportunity of communicating the results of experiments relative to the atomic weight of carbon, which Professor Erdman and myself have very lately obtained. The difference between the numbers recently given by Dumas and that of Berzelius was a sufficient inducement for us to examine and repeat Dumas's experiments, much occupied as we are with organic analysis. The burning of diamonds in oxygen gas was easily effected by us in a porcelain tube, by a pretty high temperature. The apparatus employed was very similar to that described by Dumas.

"The following are the results:—

No. 1. 0·8062 gramme diamonds left a residue weighing 0·0010 gramme, and gave 2·9467 grs. carbonic acid, consequently giving the atomic weight for carbon 75·19.

No. 2. 1·0867 gr. left a residue weighing 0·0009 gr., and gave 3·9875 grs. carbonic acid = carbon 74·84.

No. 3. 1·3575 gr. left a residue weighing 0·0018 gr., and gave 4·9659 grs. carbonic acid = carbon 75·10.

No. 4. 1·6330 gr. left a residue weighing 0·0025, and gave 5·97945 = carbon 74·98.

No. 5. 0·7510 gr. left a residue weighing 0·0010, and gave 2·7490 = carbon 75·03.

"Graphite gave the same numbers; the residues were pure white silex without a trace of oxide of iron:—

No. 1. 1·4580 gramme native graphite left a residue weighing 0·0075, and gave 5·31575 grs. = carbon 75·05 atomic weight.

No. 2. 1·5746 gr. graphite left a residue weighing 0·037, and gave 5·6377 grs. = carbon 75·02.

No. 3. 1·6578 gr. residue 0·0084, and gave 6·0385 = carbon 75·18.

No. 4. 1·9040 gr. artificial graphite, residue 0·0105 gr., gave 6·9355 grs. = carbon 75·10.

"The mean of these experiments give 75·07; we therefore consider 75 as the true number indicated by these experiments for the atomic weight of carbon. It is remarkable that this number was fixed upon theoretically by the English chemists, and has now been confirmed by experiments. If we take the number 6·239 for hydrogen, with a very small increase, viz. as 6·250, we arrive at the numbers for oxygen, carbon and hydrogen, viz. 16, 12, 2, or 8, 6, 1."

A paper was read "On Malic Acid, and the Salts of Malic Acid," by R. Hagen, M.D., translated and communicated by T. G. Tilley, Esq. This paper will be found in the present Number, p. 306.

A paper was read "On Pyroxylic Spirit," by Andrew Ure, M.D.

A paper was read "On the Ferrocyanides," by R. Corbett Campbell.

Both these papers will be inserted, entire, in a future Number of the Philosophical Magazine.

XLVII. *Intelligence and Miscellaneous Articles.*

PREPARATION OF LACTIC ACID AND LACTATES.

THE following process is recommended by MM. Boutron and E. Fremy, for preparing lactic acid and its salts:—Take about 6 pints of milk, and add to it about 8 ounces of sugar of milk dissolved in water. The mixture is to remain in an open vessel exposed to the air, for some days, at a temperature of about 60° Fahr. The liquor has then become very acid, and is to be saturated with bicarbonate of soda. In 24 to 36 hours it again becomes acid, and is to be again saturated, and so on, until all the sugar of milk is converted into lactic acid. When the conversion is reckoned complete, the milk is to be boiled to coagulate the caseum, and it is to be filtered and cautiously evaporated to the consistence of a syrup; the product of this evaporation is to be treated with alcohol, which dissolves the lactate of soda; to the alcoholic solution sufficient sulphuric acid is to be added to convert the soda into sulphate, which is precipitated, and the liquor filtered and evaporated yields lactic acid nearly pure; in order to obtain it perfectly so, it is to be saturated with chalk, and

the lactate of lime formed immediately crystallizes in perfectly white mammillated masses, and from this the lactic acid is procured by the well-known processes.

It is evident, observe the authors, that lactic acid may be saturated by other bases, and they state that a chemical manufacturer has prepared this acid from the sour water of starch-makers, by saturating it with carbonate of lime.—*Journal de Pharmacie*, xxvii. 341.

PREPARATION OF FORMIC ÆTHER.

According to Wöhler, the following process yields formic æther in large quantity, and very readily: put into a retort ten parts of starch and 37 parts of peroxide of manganese, intimately mixed in very fine powder; add to them a mixture of 30 parts of sulphuric acid, 15 parts of highly rectified spirit of wine, and 15 parts of water; the mixture is to be gently boiled, and distillation continued until the product contains no æther. In order to separate the water and the alcohol, a sufficient quantity of chloride of calcium is to be dissolved in the product, and distillation is to be performed in a water-bath; the purification is to be completed by a second rectification in the same manner.

M. Wöhler states, that in the numerous experiments which M. Kolbe made on this subject at his request, appearances presented themselves which probably indicate the presence of a peculiar substance in this æther, requiring fresh experiments for elucidation: when the fragments of fused chloride of calcium have been for some time in contact with the æther which has been once rectified, and are partly dissolved, it becomes of a deepish yellow colour, and deposits small, very fine colourless crystals on the sides of the vessel. In general the colour disappears after a certain time; it is probably owing to chloride of iron; and probably also the crystals are a compound of chloride of calcium and alcohol, which are soluble in formic æther, and may crystallize from it.—*Journal de Pharmacie*, tom. xxvii. p. 91.

PREPARATION OF IODATE OF POTASH. BY M. MELLON.

When cold no action takes place between the above-named substances; but if to chlorate of potash about three or four times its weight of distilled water be added, and then heated to ebullition, iodine added to the solution disappears in considerable quantity; the liquor is colourless, and remains so until the equivalent of iodine is exceeded; after this the liquor becomes yellow and then brown; and, as a final result, there are obtained neutral iodate of potash and chloride of iodine, with greater or less excess of the latter. If the mixture be evaporated to dryness, the chloride of iodine is disengaged, and the iodate of potash remains pure.

If the action of the iodine on the chlorate be stopped before an equivalent of the iodine is added, the liquor even then contains iodate of potash, and also chlorate [chloride ?] of iodine, corresponding unquestionably to iodic acid; for if the liquor be strongly heated,

chlorine is disengaged, and there remains chloride of iodine, which gives a precipitate of iodine with carbonate of potash.

According to M. Mellon, the formation of chloride of iodine explains the reaction that occurs: the iodine attracts the chlorine of the chlorate, whilst the greater affinity of the iodine for the oxygen, and the greater cohesion of the iodate, cause the iodine to enter into the chlorate instead of the chlorine. The action M. Mellon represents thus: $5\,Cl\,O^5, K\,O + 6I = 5\,I\,O^5, K\,O + I\,Cl^5$.

Journal de Pharmacie, tom. xxvii. p. 102.

PROFESSORSHIP OF GEOLOGY, UNIVERSITY COLLEGE, LONDON.

We have great pleasure in recording the establishment of a Chair of Geology in University College, and that Thomas Webster, Esq., formerly Secretary to the Geological Society, has been appointed to occupy it.

Professor Webster's Course on Geology at the College, consisting of Thirty Lectures, will commence on the first Tuesday in February 1842, and will be divided into three parts.

1. The description of such simple minerals as enter essentially into the composition of rocks.

2. Geology, properly so called; or the characters and superposition of the strata composing the crust of the globe, together with an account of the various phænomena exhibited by them.

3. The application of the above subjects to the useful arts.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1841.

Chiswick.—August 1. Slight rain: cloudy and fine. 2. Fine with clouds: rain. 3. Hazy: cloudy and mild: rain. 4. Cloudy and fine. 5. Fine: slight rain. 6, 7. Fine. 8. Rain: cloudy and fine. 9. Very fine. 10. Very fine: rain. 11. Stormy and wet. 12. Fine. 13. Cloudy. 14. Rain: showery: clear at night. 15—17. Cloudy and fine. 18. Hazy: fine. 19, 20. Very fine. 21. Cloudy. 22. Cloudy: slight rain. 23. Rain: cloudy and fine. 24. Showery: clear. 25. Drizzly. 26. Hazy and mild. 27. Heavy dew: cloudy and hot. 28—30. Foggy in the mornings: very fine: evenings clear. 31. Overcast and fine.

Boston.—August 1. Fine: rain P.M. 2. Fine. 3. Cloudy: rain P.M. 4. Fine: rain early A.M. 5. Cloudy: rain P.M. 6. Cloudy and stormy. 7. Cloudy: rain P.M. 8, 9. Cloudy. 10. Fine. 11. Cloudy: rain early A.M.: rain P.M. 12. Stormy. 13. Cloudy. 14. Cloudy: rain early A.M. 15, 16. Cloudy. 17. Fine: rain P.M. 18, 19. Fine. 20. Fine: thermometer 77° half-past two P.M. 21. Fine: rain A.M. 22. Fine. 23, 24. Fine: rain early A.M. 25. Rain: rain early A.M. 26. Cloudy: thermometer 75° three-quarters past two P.M. 27. Fine: thermometer 75° quarter-past eleven A.M. 28, 29. Fine. 30, 31. Cloudy.

Applegarth Manse, Dumfries-shire.—August 1. Fair, but cool and cloudy. 2. Fair and fine. 3. Wet A.M.: cleared and was fine. 4. Fair and fine. 5. Rain all day. 6. Wet A.M.: cleared and was fine. 7. Wet, slightly. 8. Fine though showery: thunder. 9. Wet A.M.: became fine. 10. Showery. 11. Fair. 12. Showery all day. 13. Partial showers. 14. Wet A.M.: became fine. 15. Fine till P.M.: then rain. 16. Wet A.M.: cleared P.M. 17. Fair throughout. 18. Fair A.M.: wet P.M. 19. Fair and warm: air electrical. 20. Wet nearly all day: thunder. 21. Wet P.M.: flood. 22. Fine and fair. 23. Occasional slight showers. 24. Wet P.M. and evening: thunder. 25. Showery. 26. Rain early A.M.: cleared. 27. Fine: one shower A.M. 28. Wet morning: cleared. 29. Fine but cloudy. 30. Wet all day. 31. Fair and fine.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Aug.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
1.	29·830	29·923	29·790	29·30	29·68	29·75	58·3	65·2	50·0	66	49	57	61	49	NW.	SW.	calm	WNW.	·055	·02	...	...	54
○ 2.	29·986	29·937	29·765	29·42	29·75	29·67	62·5	64·0	54·0	70	58	61	62½	44	WSW.	SW.	NW.	SW.	·005	·38	·05	...	55
3.	29·742	29·691	29·411	29·16	29·53	29·38	62·8	70·8	60·4	73	57	64·5	63	53½	NNW.	SW.	calm	SSW.	·377	·38	...	...	59
4.	29·492	29·785	29·385	28·94	29·53	29·60	60·0	71·0	59·0	69	57	63	65½	51	N.	N.	N.	S.	·366	...	·86	...	58
5.	29·796	29·742	29·609	29·17	29·42	29·21	65·0	69·0	60·0	64	57	64	71	53	SW.	SW.	SE.	E.	...	·01	...	...	61
6.	29·748	29·851	29·663	29·05	29·34	29·55	60·6	66·0	58·5	67	57	62	60	54	W.	W.	W.	W.	·008	·07	·06	...	57
7.	29·962	29·893	29·798	29·33	29·59	29·50	64·0	69·5	60·5	74	58	62·5	61	52½	W.	W.	calm	SSW.	·088	...	...	1·47	57
8.	29·738	29·716	29·619	29·12	29·38	29·41	61·0	71·4	59·5	67	52	63	60	55½	SSW.	S.	W.	SW.	·008	·01	·01	...	59
9.	29·646	29·684	29·596	29·04	29·39	29·38	59·5	69·0	56·0	70	49	60	59	50½	W.	W.	calm	W.	...	·01	...	...	57
☾ 10.	29·874	29·800	29·678	29·26	29·49	29·58	61·0	72·5	53·0	68	55	61·5	58	48	SW.	S.	SW.	SW.	...	·36	...	...	56
11.	29·512	29·662	29·458	28·95	29·49	29·56	61·4	67·5	58·0	68	46	60	60	45	SW.	SW.	S.	NW.	·277	·30	·61	...	59
12.	29·958	29·923	29·888	29·34	29·70	29·74	59·2	68·0	51·0	65	41	57	61	44	W var.	NW.	W.	NNW.	·080	...	·45	...	51
13.	29·928	29·886	29·772	29·36	29·65	29·61	62·0	64·0	61·5	70	54	60	61	42	W.	S.	W.	E.	·013	·30	...	...	56
14.	29·646	29·719	29·599	29·08	29·39	29·44	62·0	66·5	57·0	72	54	61	62½	53	SW.	SW.	SW.	SW.	·213	·04	·25	0·28	58
15.	29·766	29·746	29·687	29·16	29·49	29·62	64·5	68·6	57·0	70	50	61·5	67	55	W.	SW.	calm	W.	·036	...	...	...	59
● 16.	29·928	29·921	29·851	29·32	29·72	29·72	61·5	71·6	56·0	74	58	60	64	45	WNW.	S.	calm	SW.	·011	...	...	...	57
17.	30·018	30·056	29·969	29·35	29·73	29·94	64·5	71·5	60·0	75	56	63·5	62½	54	SW.	W.	W.	NW.	...	...	...	...	60
18.	30·210	30·173	30·140	29·61	30·00	30·04	63·7	73·0	58·4	76	56	62·5	64	43½	NW.	W.	calm	NW.	...	...	·01	...	60
19.	30·276	30·197	30·006	29·60	30·00	29·95	63·5	69·5	56·0	77	47	61	61½	53	WSW.	SW.	calm	S.	...	...	...	...	57
20.	30·006	29·734	29·629	29·37	29·75	29·64	66·5	73·0	56·0	80	51	66	69	58	SSE.	W.	S.	W.	...	...	...	...	59
21.	29·684	29·903	29·797	29·00	29·55	29·52	66·7	77·5	53·0	72	48	67	61	49	W.	S.	SE.	NW.	...	...	...	2·00	63
22.	29·978	29·874	29·800	29·36	29·65	29·48	63·0	73·0	56·0	68	54	59	61	48½	SW.	S.	calm	E.	...	·58	·12	...	56
☽ 23.	29·904	29·884	29·797	29·30	29·60	29·78	58·0	68·0	57·0	72	44	57	63	49	WNW.	W.	calm	W.	·416	·01	·18	...	57
24.	30·124	30·094	30·048	29·36	29·94	29·94	57·0	67·5	50·5	67	45	56	61½	41½	WNW.	N.	NW.	WNW.	·015	·13	·04	...	48
25.	30·148	30·079	30·059	29·44	29·83	29·90	57·0	65·0	52·0	67	60	55·5	64½	50	SW.	SW.	NW.	SW.	·130	·07	·21	...	...
26.	30·268	30·225	30·181	29·59	30·00	30·10	63·2	65·5	57·0	79	62	64	63	55	W.	SW.	calm	SW.	·061	...	...	...	61
27.	30·332	30·244	30·142	29·59	30·07	30·00	69·7	70·0	65·0	81	51	69	65½	56½	W.	SW.	calm	SW.	·011	...	...	...	64
28.	30·264	30·189	30·125	29·54	30·01	30·03	62·7	76·5	59·5	77	52	62·5	65	54	W.	W.	calm	W.	...	...	...	1·26	59
29.	30·236	30·159	29·920	29·55	29·92	29·85	65·4	72·5	59·0	81	51	65	66	54½	WSW.	SW.	calm	SW.	...	...	...	...	62
30.	30·064	29·998	29·840	29·35	29·73	29·60	63·5	77·0	60·0	80	67	65	61	56½	W.	S.	calm	SSW.	...	...	...	...	60
31.	29·826	29·844	29·729	29·16	29·69	29·80	66·7	76·0	63·0	74	45	65	62	50½	W.	NW.	calm	WNW.	...	·02	...	...	63
Mean.	29·932	29·920	29·798	29·29	29·680	29·716	62·5	70·0	57·2	72·03	52·93	61·8	62·1	50·5					Sum. 2·168	2·69	2·85	5·01	Mean. 58

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XLVIII. *On the Chemical Statics of Organized Beings.* *By* M. Dumas.

[The discourse of which the following is a translation, attracted, as we are informed, much attention, on its delivery by M. Dumas. It formed the concluding Lecture of his course at the *Ecole de Médecine*, and has just been published.—Ed.]

LIFE, whose painful mysteries you are called upon to fathom, exhibits among its phænomena some which are manifestly connected with the forces that inanimate nature herself brings into action, others which emanate from a more elevated source, less within the reach of our boldest stretch of thought.

It has not been my province to accompany you in looking with an inquisitive eye into all that part of your studies under which those facts which appertain to the normal or irregular exercise of the instincts of life arrange themselves. Still less have we ever had to bring under our consideration those noble faculties, by means of which the human intellect, mastering all that surrounds it, breaking down all obstacles, bending all the powers of nature to its wants, has step by step made conquest of the earth, of the seas, of the whole globe;—a vast domain, which our recollections, our presentiments perhaps, so often make us consider as too narrow a prison. To others more fortunate belongs the care of initiating you in these important studies, the privilege of unfolding to you these lofty themes; our task, more humble, must be limited to the field of the physical phænomena of life; and there are still some which have not found a place in our lectures.

It is specially, indeed, the functions of matter in the production and growth of organized beings, the part which it

takes in the accomplishment of the phænomena of their daily existence, the alterations which it undergoes after their death, that we have had to study together, and this study alone has quite sufficed us for this year's occupation.

I.—Plants, animals, man, contain matter. Whence comes it? What does it effect in their tissues and in the fluids which bathe them? What becomes of it when death breaks the bonds by which its different parts were so closely united?

These are the questions which we touched upon together, at first, with hesitation, for the problem might be far above the powers of modern chemistry; we afterwards considered them with somewhat more confidence, as we felt from the silent and inward assent of our understandings that the path was sure, and that we could descry the goal gradually standing out, clear of all that obstructed our vision. If from these labours, which you have witnessed, or I should rather say, in which you have taken part; if from this scientific effort there have arisen some general views, some simple formulæ, it is my duty to become their historian; but allow me the pleasure of adding, that they belong to you, that they belong to our school, the intelligence of which has been exercised on this new ground. It is the ardour with which you have followed me in this career that has given me strength to pursue it; it is your interest which has sustained me; your curiosity which has awakened mine; your confidence which has made me see, and which proves to me at this moment that we are still in the path of truth.

These remarks will remind you of the wonder with which we found, that of the numerous elements of modern chemistry, organic nature borrows but a very small number; that from these vegetable or animal matters, now multiplied to infinity, general physiology borrows not more than from ten to twelve species; and that all the phænomena of life, so complicated in appearance, belong, essentially, to a general formula so simple, that, so to speak, in a few words the whole is stated, the whole summed up, the whole foreseen.

Have we not proved, in fact, by a multitude of results, that animals constitute, in a chemical point of view, a real apparatus for combustion, by means of which burnt carbon incessantly returns to the atmosphere under the form of carbonic acid; in which hydrogen burnt without ceasing, on its part continually engenders water; whence, in fine, free azote is incessantly exhaled by respiration, and azote in the state of oxide of ammonium by the urine?

Thus from the animal kingdom, considered collectively, constantly escape carbonic acid, water in the state of vapour, azote, and oxide of ammonium,—simple substances, and few in

number, the formation of which is strictly connected with the history of the air itself. Have we not, on the other hand, proved that plants, in their normal life, decompose carbonic acid for the purpose of fixing its carbon and of disengaging its oxygen; that they decompose water to combine with its hydrogen, and to disengage also its oxygen; that, in fine, they sometimes borrow azote directly from the air, and sometimes indirectly from the oxide of ammonium, or from nitric acid, thus working in every case in a manner the inverse of that which is peculiar to animals? If the animal kingdom constitutes an immense apparatus for combustion, the vegetable kingdom, in its turn, constitutes an immense apparatus for reduction, in which reduced carbonic acid yields its carbon, reduced water its hydrogen, and in which also reduced oxide of ammonium and nitric acid yield their ammonium or their azote.

If animals then continually produce carbonic acid, water, azote, oxide of ammonium,—plants incessantly consume oxide of ammonium, azote, water, carbonic acid. What the one class of beings gives to the air, the others take back from it; so that to take these facts at the loftiest point of view of terrestrial physics, we must say that, as to their truly organic elements, plants and animals spring from air,—are nothing but condensed air; and that, in order to form a just and true idea of the constitution of the atmosphere at the epochs which preceded the birth of the first organized beings on the surface of the globe, there must be placed to the account of the air, by calculation, that carbonic acid and azote, whose elements have been appropriated by plants and animals. Thus plants and animals come from the air, and thus to it they return; they are real dependences of the atmosphere.

Plants, then, incessantly take from the air what is given to it by animals; that is to say, carbon, hydrogen and azote, or rather, carbonic acid, water and ammonia.

It now remains to be stated, how in their turn, animals acquire those elements which they restore to the atmosphere; and we cannot see without admiring the sublime simplicity of all these laws of nature, that animals always borrow these elements from plants themselves.

We have, indeed, ascertained, from the most satisfactory results, that animals do not create true organic matters, but that they destroy them; that plants, on the contrary, habitually create these same matters, and that they destroy but few of them, and that in order to effectuate particular and determinate conditions.

Thus it is in the vegetable kingdom that the great laboratory of organic life resides; there it is that the vegetable and

animal matters are formed, and they are there produced at the cost of the air:

From vegetables, these matters pass ready-formed into the herbivorous animals, which destroy a portion of them, and accumulate the remainder in their tissues:

From herbivorous animals, they pass ready-formed into the carnivorous animals, who destroy or retain some of them according to their wants:

Lastly, during the life of these animals, or after their death, these organic matters, as they are destroyed, return to the atmosphere whence they proceeded.

Thus closes this mysterious circle of organic life at the surface of the globe. The air contains or engenders oxidized products, as carbonic acid, water, nitric acid, oxide of ammonium. Plants, constituting true reducing apparatus, possess themselves of their radicals, carbon, hydrogen, azote, ammonium. With these radicals they form all the organic or organizable matters which they yield to animals. These, forming, in their turn, true apparatus for combustion, reproduce carbonic acid, water, oxide of ammonium and nitric acid, which return to the air to produce anew and through endless ages the same phænomena.

And if we add to this picture, already, from its simplicity and its grandeur, so striking, the indisputable function of the solar light, which alone has the power of putting in motion this immense apparatus,—this apparatus never yet imitated, constituted of the vegetable kingdom, and in which is accomplished the reduction of the oxidized products of air,—we shall be struck with the import of these words of Lavoisier:—

"Organization, sensation, spontaneous movement, life, exist only at the surface of the earth, and in places exposed to the light. It would seem that the fable of the torch of Prometheus was the expression of a philosophic truth which had not escaped the ancients. Without light, nature was without life, was dead and inanimate: by the gift of light, a beneficent God spread upon the surface of the earth organization, feeling and thought."

These words are as true as they are beautiful. If feeling and thought, if the noblest faculties of the soul and of the intellect, have need, for their manifestation, of a material covering, to plants is assigned the framing of its web with the elements which they borrow from the air, and under the influence of the light which the sun, its inexhaustible source, pours in unceasing floods upon the surface of the globe.

And as if, in these great phænomena, all must be connected with causes which appear the most distant from them, we must

moreover remark how the oxide of ammonium, the nitric acid, from which plants borrow a part of their azote, are themselves almost always derived from the action of the great electric sparks which flash forth in stormy clouds, and which (furrowing the air through a vast extent) produce there the nitrate of ammonia which analysis detects in it.

Thus, from the craters of those volcanos whose convulsions so often agitate the crust of the globe, continually escapes carbonic acid, the principal nutriment of plants; from the atmosphere flashing with lightnings, and from the midst of the tempest itself, there descends upon the earth the other and no less indispensable nutriment of plants, that whence they derive almost all their azote, the nitrate of ammonia, contained in storm-showers.

Might not this be called, as it were, an idea of that chaos of which the Bible speaks, of those times of disorder and of tumult of the elements, which preceded the appearance of organized beings upon the earth?

But scarcely are the carbonic acid and the nitrate of ammonia produced, than a form more calm, although not of inferior energy, comes to put them in action,—it is Light. Through her influence, the carbonic acid yields its carbon, the water its hydrogen, and the nitrate of ammonia its azote. These elements unite, organized matters form, and the earth puts on its rich carpet of verdure.

It is then by continually absorbing a real force, the light and the heat emanating from the sun, that plants perform their functions, and that they produce this immense quantity of organized or organic matter, pasture destined for the consumption of the animal kingdom.

And if we add, that animals on their part produce heat and force in consuming what the vegetable kingdom* has produced and has slowly accumulated, does it not seem that the ultimate end of all these phænomena, their most general formula, reveals itself to our sight?

The atmosphere appears to us as containing the primary substances of all organization, volcanos and storms as the laboratories in which were first produced the carbonic acid and the nitrate of ammonia which life required for its manifestation or its multiplication.

In aid of these comes light, and developes the vegetable kingdom,—immense producer of organic matter: plants absorb the chemical force which they derive from the sun to decompose carbonic acid, water and nitrate of ammonia;

[* "*Le règne animal*" in the original; but this is obviously an error.—Edit.]

as if plants realized a reducing apparatus superior to all those with which we are acquainted; for none of these would decompose carbonic acid in the cold.

Next come animals, consumers of matter and producers of heat and force, true apparatus for combustion. It is in them undoubtedly that organized matter puts on its highest expression. But it is not without suffering from it that it becomes the instrument of sensation and of thought; under this influence organized matter undergoes combustion; and in reproducing the heat and the electricity, which produce our strength and which are the measure of its power, these organized or organic matters become annihilated in order to return to the atmosphere whence they came. Thus the atmosphere constitutes the mysterious link which binds the vegetable to the animal kingdom.

Vegetables then absorb heat and accumulate matter which they have the power to organize.

Animals, through whom this organized matter only passes, burn or consume it in order to produce in its aid the heat and the different powers which their movements turn to account.

Suffer me, therefore, if, borrowing from modern sciences an image of sufficient magnitude to bear comparison with these great phænomena, we should liken the existing vegetation,—truly a storehouse in which animal life is fed,—to that other storehouse of carbon constituted of the ancient deposits of pit-coal, and which, burnt by the genius of Papin and of Watt, also produces carbonic acid, water, heat, motion,—one might almost say life and intelligence.

In our view, therefore, the vegetable kingdom will constitute an immense depôt of combustible matter destined to be consumed by the animal kingdom, and in which the latter finds the source of the heat and of the locomotive powers of which it avails itself.

Thus we observe a common tie between the two kingdoms, the atmosphere; four elements in plants and in animals, carbon, hydrogen, azote and oxygen; a very small number of forms under which vegetables accumulate them, and under which animals consume them; some very simple laws, which their connexion simplifies still more; such would be the picture of the most elevated state of organic chemistry which would result from our conferences of the present year.

You, like myself, have felt, that before separating we have need of collecting our thoughts, of fixing with precision all the facts, of bringing together and summing up the opinions which explain and develop these great principles; lastly, that

it was useful, as regarded your future studies, to give you in writing and in a clearer form the expression of these views, which were partly brought into existence under the stimulus of your presence, and consequently reduced into form with the hesitation which so often accompanies the first enunciation of our thoughts.

II.—Since [the causes of] all the phænomena of life are exerted upon matters which have for their base carbon, hydrogen, azote, oxygen; since these matters pass over from the animal kingdom to the vegetable kingdom by intermediary forms, carbonic acid, water, and the oxide of ammonium; lastly, since air is the source whence the vegetable kingdom is fed, and the reservoir in which the animal kingdom is annihilated; we are led to take a rapid survey of these different bodies with a special view to general physiology.

Composition of Water.—Water is incessantly formed and decomposed in animals and plants; to appreciate what results from this, let us first see how it is composed. Some experiments founded on the direct combustion of hydrogen, and in which I have produced more than two pounds of artificial water,—experiments which are in truth very difficult and very delicate, but in which any errors would be unimportant with regard to the circumstances which we are engaged upon,—make it very probable that water is formed, in weight,

Of 1 part hydrogen
And 8 parts oxygen,

and that these whole and simple numbers express the true relation according to which these two elements combine to form water.

As substances always present themselves to the eyes of the chemist by molecules, as he always endeavours to connect in his thoughts, with the name of each substance, the weight of the molecule, the simplicity of this relation is not unimportant.

In fact, each molecule of water being formed of one molecule of hydrogen and one molecule of oxygen, we arrive at these simple numbers, which cannot be forgotten.

A molecule of hydrogen weighs 1; a molecule of oxygen weighs 8; and a molecule of water weighs 9.

Composition of Carbonic Acid.—Carbonic acid keeps incessantly forming in animals, and is continually undergoing decomposition in plants; its composition, therefore, deserves a special notice in its turn.

Now carbonic acid, like water, is represented by the most simple numbers.

Experiments founded on the direct combustion of the dia-

mond, and on its conversion into carbonic acid, have proved to me that this acid is formed of the combination of 6 parts by weight of carbon and 16 parts by weight of oxygen.

We are therefore led to represent carbonic acid as being formed of one molecule of carbon weighing 6, and two molecules of oxygen weighing 16, which constitute one molecule of carbonic acid weighing 22.

Composition of Ammonia.—Lastly, ammonia, in its turn, seems formed in whole numbers of 3 parts of hydrogen and 14 of azote, which may be represented by 3 molecules of hydrogen weighing 3, and by 1 molecule of azote weighing 14.

Thus, as if the better to show all her power, nature operates, in the business of organization, upon a very small number only of elements, combined in the most simple proportions.

The atomic system of the physiologist revolves on these four numbers: 1, 6, 7, 8.

1, is the molecule of hydrogen;
6, that of carbon;
7, or twice 7, i. e. 14, that of azote;
8, that of oxygen.

These numbers should always be associated with these names, because for the chemist there can exist no abstract hydrogen, nor carbon, nor azote, nor oxygen. They are beings in their reality which he has always in view; it is of their molecules that he always speaks; and to him the word hydrogen depicts a molecule which weighs 1; the word carbon, a molecule which weighs 6; and the word oxygen, a molecule which weighs 8.

Composition of the Air.—Does atmospheric air, which performs so great a part in organic nature, also possess as simple a composition as water, carbonic acid and ammonia? This is the question which M. Boussingault and I have recently been studying. Now, we have found that, as the greater number of chemists have thought, and contrary to the opinion of Dr. Prout, to whom chemistry owes so many ingenious views, air is a mixture,—a true mixture.

In weight, air contains 2,300 of oxygen for 7,700 of azote; in volume, 208 of the first for 792 of the second.

The air, besides, contains from 4 to 6–10,000ths of carbonic acid in volume, whether it be taken at Paris or in the country. Ordinarily, it contains 4–10,000ths.

Moreover, it contains a nearly equal quantity of the carburetted hydrogen gas which is called marsh gas, and which stagnant waters disengage perpetually.

We do not speak of aqueous vapour, which is so variable;

of oxide of ammonium and of nitric acid, which can only have a momentary existence in the air because of their solubility in water.

The air, then, is constituted of a mixture of oxygen, azote, carbonic acid, and marsh gas.

The carbonic acid in it varies, and indeed greatly, since the differences in it extend almost from the simple to the double, from 4 to 6–10,000ths. May not this be a proof that plants take from the air this carbonic acid, and that animals take back a part from it? in a word, may this not be a proof of that equilibrium of the elements of the air attributed to the inverse actions which animals and plants produce upon it?

It has, indeed, been long since remarked; animals borrow from the air its oxygen, and give to it carbonic acid; plants, in their turn, decompose this carbonic acid in order to fix its carbon and restore its oxygen to the air.

As animals breathe continually; as plants breathe under the solar influence only; as in winter the earth is stript, whilst in summer it is covered with verdure; it has been supposed that the air must transfer all these influences into its constitution.

Carbonic acid should augment by night and diminish by day. Oxygen, in its turn, should follow an inverse progress.

Carbonic acid should also follow the course of the seasons, and oxygen obey the same law.

All this is true, without doubt, and quite perceptible as to a portion of air limited and confined under a jar; but in the mass of the atmosphere, all these local variations blend and disappear. Accumulated centuries are requisite in order effectually to put in action this balance of the two kingdoms, with regard to the composition of air; we are then very far from those daily or yearly variations, which we had been apt to look upon as being as easy to observe as to foresee.

With regard to oxygen, calculation shows that, exaggerating all the data, not less than 800,000 years would be required for the animals living on the surface of the earth to consume it entirely.

Consequently, if we suppose that an analysis of the air had been made in 1800, and that during the entire century plants had ceased to perform their functions on the surface of the whole globe, the animals at the same time all continuing to live, the analysts in 1900 would find the oxygen of the air diminished by 1–8000th of its weight, a quantity which is beyond the reach of our most delicate methods of observation, and which, assuredly, would have no influence whatever on the life of animals or plants.

As to this, then, we cannot be deceived; the oxygen of the

air is consumed by animals, who convert it into water and carbonic acid; it is restored by plants, which decompose these two bodies.

But nature has arranged everything so that the store of air should be such with relation to the consumption of animals, that the want of the intervention of plants for the purification of the air should not be felt until centuries have elapsed.

The air which surrounds us weighs as much as 581,000 cubic kilometres of copper; its oxygen weighs as much as 134,000 of these same cubes. Supposing the earth peopled with a thousand millions of men, and estimating the animal population at a quantity equivalent to three thousand millions of men, we should find that these quantities united consume in a century only a weight of oxygen equal to 15 or 16 cubic kilometres of copper, whilst the air contains 134,000 of it.

It would require 10,000 years for all these men to produce a perceptible effect upon the eudiometer of Volta, even supposing vegetable life annihilated during all this time.

In regard to the permanence of the composition of air, we may say with all confidence, that the proportion of oxygen which it contains is secured for many centuries, even reckoning for nothing the influence of vegetables, and that, nevertheless, these restore oxygen to it incessantly in quantity at least equal to that it loses, and perhaps more; for vegetables live just as much at the expense of the carbonic acid furnished by volcanos, as at the expense of the carbonic acid furnished by animals themselves.

It is not then for the purpose of purifying the air that these breathe, that vegetables are especially necessary to animals; it is, above all, to furnish them, incessantly, with organic matter quite ready for assimilation; organic matter, which they may burn to their advantage.

There is, therefore, a service necessary, without doubt, but so remote, that it can scarcely be recognized, which vegetables render us, in purifying the air which we consume. There is another service so immediate, that if, during a single year, it were to fail us, the earth would be depopulated; it is that which these same vegetables render us by preparing our nutriment, and that of all the animal kingdom. In this, especially, is found the chain that binds together the two kingdoms. Annihilate plants, and the animals all perish of a dreadful famine; organic nature itself entirely disappears with them in a few seasons.

We have, however, said, that the carbonic acid of the air varies from 4 to 6–10,000th. These variations are very frequent and very easy to observe. Is not this a phænomenon re-

proaching the influence of animals who introduce this acid into the air, and that of vegetables, which deprive it of it?

No; this phænomenon, you are aware, is a simple meteorological phænomenon. It is with carbonic acid as with aqueous vapour, which forms on the surface of the sea, to become condensed elsewhere, fall again in rain, and be reproduced under the form of vapour.

This water, which is condensed and falls, dissolves, and carries with it carbonic acid; this water, which evaporates, yields up this same gas to the air.

A great meteorological interest would attach to the observation of the variations of the hygrometer, and those of the seasons, or of the state of the sky with the variations of the carbonic acid of the air; but hitherto all tends to show that these rapid variations constitute a simple meteorological event, and not, as had been thought, a physiological event, which, singly considered, would infallibly produce variations infinitely slower than those which are, in fact, observed as much in towns as in the country itself.

Thus the air is an immense reservoir, whence plants may for a long time derive all the carbonic acid necessary for their wants; where animals, during a much longer time still, will find all the oxygen that they can consume.

It is also from the atmosphere that plants derive their azote, whether directly or indirectly: it is there that animals finally restore it.

The atmosphere is therefore a mixture which unceasingly receives and supplies oxygen, azote or carbonic acid, by means of a thousand exchanges of which it is now easy to form a just idea, and the details of which a rapid analysis will now enable us to appreciate.

[To be concluded in our next Number.]

XLIX. *On the Periodical Shooting Stars, and on Shooting Stars in general. By* Miss Zornlin*.

Of the meteorological nature of the periodical shooting stars of August and November, little doubt, I should think, can remain in the mind of any one who has observed and considered the general character of the attendant phænomena. It may, however, be desirable to adduce a few instances in support of this assumption.

In our own island, the November periodical shooting stars

* Communicated by the Author.

have not been witnessed in the same splendour as in other lands possessing a clearer atmosphere, and perhaps a more excessive climate. Phænomena of no small degree of interest, have, however, not unfrequently been exhibited in this country; these, in some cases, not being the less important, from the evidence they afford that the periodical shooting stars cannot be regarded as an isolated meteorological phænomenon.

The most remarkable display of the November shooting stars of which I have met with any record, as occurring in Britain, took place in 1832; and as I believe that the phænomena here referred to have not hitherto been brought forward as connected with the periodical shooting stars, it may not be uninteresting to insert some notice of them in this place. Nor are these accounts less worthy of notice, from the circumstance of their having been recorded before public attention had been directed to the probable periodicity of the phænomenon, by the grand display of 1833. A paper was read before the Royal Society on the 13th of December, 1832, entitled 'An Account of an extraordinary Meteor seen at Malvern, November 12, 1832,' by W. Addison, Esq., F.L.S., "in which the author states that he beheld, from the Malvern Hills, a constant succession of meteors, of various degrees of magnitude and brilliancy. The smaller ones were like those commonly called shooting stars; others were much more brilliant; and notwithstanding the bright moonshine, threw a strong glare upon every object: they also left behind them a long train of very vivid white light. The author witnessed this scene for upwards of an hour, and it was still going on when he left it. At one time he counted forty-eight of these meteors during the interval of five minutes*." In the London Literary Gazette of the same year, we find the following account:—"It appears from the provincial newspapers, in various parts of the country, that very remarkable phænomena were seen, both north and south, on the morning of Tuesday week [November 13th]. Fiery meteors and falling stars (as they are called) issued from the west, and illuminated the heavens in their course, leaving behind them trains of brilliant white. The appearances seem to have been very grand, and to have excited much admiration in the beholders." From these accounts, it must be inferred that the display of the periodical shooting stars on this occasion was very splendid; and also of considerable duration, commencing, as it evidently did, on the evening of the 12th, and extending to the morning of the 13th of November. The phænomenon

* Abstracts of the Philosophical Transactions, vol. iii. p. 159. See also Phil. Mag. for July 1833. [Third Series, vol. iii. p. 37.]

was also witnessed, at the same time, in considerable splendour, at Geneva, and in other parts of Europe; at Orenburg, and in Arabia.

The November periodical shooting stars have likewise, as is well known, been observed in later years in this country, though in smaller numbers; but what is more to my present purpose, on some occasions, whilst a brilliant display of these meteors has been witnessed in other parts of the globe, meteoric phænomena of different character have been observed in this island. Thus, on the evening of November 12, 1837, a fine coloured aurora, accompanied by some shooting stars, was observed at this place, as well as in many other parts of Britain *; and on the morning of the 13th of November, 1838, I witnessed meteoric phænomena of the grandest description, consisting of three successive and distinct splendid auroras, exhibiting well-defined arches, as also streamers and patches of the most vivid red and green hues. This magnificent display was accompanied by a small number of shooting stars †. Auroras, and other similar meteoric phænomena, are also on record as having occurred in various years on the 12th and 13th of November in other parts of the globe. Thus, on the 13th of November, 1832, when, as already mentioned, shooting stars were observed in great splendour in this country, as also at Geneva, in Arabia, and at Orenburg, remarkable meteoric phænomena, consisting of brilliant luminous columns and bands, were witnessed both to the north and south of the latter place; and on the 13th of November, 1836, an aurora was observed at New York, and other places in the United States. Nor does the occurrence of these periodical meteoric phænomena appear to be confined to very recent times; for we find it recorded, that on November 13, 1755, "The sky was red, and red rain fell in different countries." And again, on the 14th of November, 1765, "Red rain fell in Picardy ‡." Nay, the collection of facts from which the above accounts are taken, enables us to trace the occurrence of apparently similar phænomena to a much earlier era; it being recorded that on the 5th or 6th of November, 472, there was "a great fall of black dust, probably at Constantinople, during which the heavens

* See an account by Professor Forbes in Phil. Mag., Supplementary Number for January 1838. [Third Series, vol. xii. p. 85.]

† For a notice of the phænomena exhibited on this occasion, I would refer to a paper in the Phil. Mag. for Jan. 1839, [S. 3. vol. xiv. p. 39.] by Mr. W. R. Birt, with whose observations my own, as far as they were carried, closely correspond; though no description could convey an adequate idea of the splendour of the phænomena.

‡ Account of Meteoric Stones, &c. which have fallen from the Heavens, from the earliest period down to 1819. Edin. Phil. Journal, Oct. 1819.

seemed to burn." The latter date, allowing for the difference of style in that age, would form a close approximation to the present 12th or 13th day of the month.

Similar in character to the November shooting stars, are those of August; and, like the former, they also are accompanied by other meteoric phænomena. Thus, on both occasions, lightning has frequently been noticed; and luminous appearances in the clouds were observed by myself on the night of August 11, 1839 *, much resembling those noticed by M. Wartmann at Geneva, on the night of November 12, 1836 †. And in the Sixth Report of the British Association for the Advancement of Science, we meet with an account, communicated by Dr. Traill, of a splendid aurora borealis, which was observed on August 11, 1836. A very remarkable instance of the August periodical meteors, which is mentioned in Colonel Reid's work 'On the Law of Storms,' may be worth citing in this place ‡. The phænomenon occurred during the hurricane at Barbadoes in 1831, and the relation is given in an extract from an account published at Bridgetown in that island, immediately after it occurred. The hurricane had been raging in all its fury, accompanied by incessant lightning. At about three o'clock a.m. on the 11th of August, a temporary cessation in the violence of the wind occurred. "The lightning having also ceased, for a few moments only at a time, the blackness in which the town was enveloped was inexpressibly awful. Fiery meteors were presently seen falling from the heavens; one in particular, of a globular form, and a deep red hue, was observed by the writer to descend perpendicularly from a vast height. It evidently fell by its specific gravity, and was not shot or propelled by any extraneous force. On approaching the earth with accelerated motion, it assumed a dazzling whiteness, and an elongated form, and dashing to the ground in Beckwith Square, opposite the stores of Messrs. U. D. Grierson and Co., it splashed around in the same manner as melted metal would have done, and was instantly extinct: its brilliancy, and the spattering of its particles on the earth, gave it the resemblance of a body of quicksilver of equal bulk." Shortly afterwards, the lightning recurred in terrific grandeur, and the hurricane in increased violence §.

* Phil. Mag., December 1839. [S. 3. vol. xv. p. 441.]

† Phil. Mag., September 1837. [S. 3. vol. xi. p. 261.]

‡ Some of the severest storms on record have occurred in the months of August and November. It would form an interesting subject of inquiry, whether such storms have any connexion with the appearance or the non-appearance of the periodical meteoric phænomena at those times.

§ Lieut.-Col. Reid on the Law of Storms, 1st edit., p. 29.

It would be easy to multiply instances of the periodical return both of the November and August shooting stars, but the periodicity of these meteors I consider as granted; and, with the exception of two remarkable instances, neither of which I believe has appeared in any published catalogue, and which I have therefore been induced to insert, my object has been solely to bring forward facts in support of the opinion that the display of the periodical shooting stars cannot be regarded as an isolated meteorological phænomenon.

The nature and origin of the periodical, and indeed of all other shooting stars, appears still to be involved in much obscurity. From the above, and similar facts, it may, however, be inferred, that they are connected with other meteorological phænomena—phænomena which apparently originate in electricity, and which may be considered as atmospheric. We shall therefore be led to seek for their origin in our own atmosphere, and shall not surely be wandering from the mark, if to electricity we refer the production of these, and perhaps of all meteors of similar character.

The visible effects of electricity (whether voltaic or ordinary) are, according to Mr. Faraday, "the evolution of heat, the production of magnetism, chemical decomposition, physiological changes, and lastly, the evolution of light, in the form of a spark." Among the visible effects of electricity in motion, are also included the luminous appearances presented by the aurora borealis. And may not shooting stars originate in electric currents more energetically developed?

According to the law discovered by Mr. Faraday, "the decomposing action of any current of electricity is constant for a constant quantity of electricity;" and as the same philosopher has proved by experiment, "a given quantity of electricity, whether passed in one or in many portions, invariably decomposes the same quantity of water." That a similar constancy will exist in the decomposing action of currents of electricity which may be evolved in the atmosphere, cannot be questioned; and that effects analogous to those elicited by the experiments made by Mr. Faraday on the decomposition of water should be produced in the atmosphere by such electric currents, is more than probable. If, then, we may suppose the decomposition of water into its constituent elements to be in progress in the atmosphere, we must also look for some reaction, some antagonist power to counterbalance such a destructive process, some agent by which the reproduction of water may be effected. And may not electricity, under some circumstances, be evolved to so

great a degree in the atmosphere*, as to cause the compression and combination of the elements of oxygen and hydrogen? a combination which would be accompanied by ignition and explosion, and by the formation of water. May not such ignition and explosion produce the luminous appearances called shooting stars?

Processes of this description might be in progress at all elevations in the atmosphere to which oxygen and hydrogen (and perhaps, in some cases, nitrogen also) can extend or be forced in contact; and consequently, wherever aqueous vapour exists to be decomposed, that is, probably, to the utmost limits of our atmosphere; and perhaps these gases may even occasionally be driven by electric currents beyond its ordinary limits. If such an explosion should take place beyond these limits, or even in very elevated strata of the atmosphere, it would necessarily be noiseless; but if within a short distance of the earth's surface, a detonation might be heard. The latter phænomena might also, in some instances, appear of large dimensions, and occasionally (as in the case of the meteor already alluded to at Barbadoes) approach the ground; or even (as in the instance of the large and brilliant meteor which fell on the 13th of November, 1835, and set fire to a barn near Belley, in the Départementde l'Ain†,) might sometimes cause the ignition of combustible substances. The apparent balls of fire, occasionally witnessed during thunder-storms, may, perhaps, be of similar or nearly similar origin. And in these meteors we might also find an explanation of the remarkable phænomenon of thunder in a clear and serene sky, alluded to by M. Arago in the *Annuaire* for 1838, and observed by Mr. Addison at Great Malvern on the 4th of August, 1835, when "a remarkable noise was heard at 4 p.m., like a loud clap of thunder, the air at the time being quite free from cloud, and the sun hot and brilliant‡." Nor is it impossible, that to a somewhat similar cause may be attributed the phænomena of water-spouts, and perhaps also of whirlwinds§; for Captain Beechey, speaking of the former, relates that on one occasion

* The remarkable fact, very recently observed, of the production of electricity by the jets of steam issuing from boilers, and the instance it gives of the evolution of electricity upon an enormous scale, during the conversion of water into vapour, (see Phil. Mag., November 1840, S. 3. vol. xvii. p. 370.) tends to confirm and support the views here advanced.

† *Annuaire du Bureau des Longitudes*, 1836.

‡ Report to the British Association for the Advancement of Science, 1839.

§ It is, at least, a singular coincidence, that the remarkable phænomenon of moving pillars of sand, as described by Bruce, in Nubia, occurred on the 14th and 15th of November.

"a ball of fire was observed to be precipitated into the sea;" and Colonel Reid mentions that "many of the descriptions of these whirlwinds speak of visible flame attending them, and occasionally of a remarkable noise." If we suppose the portion of ignited matter to consist solely of oxygen and hydrogen, the result would be pure water; and, when formed in elevated strata of the atmosphere, no trace evident to our senses might remain (unless, indeed, when these meteors should occur in vast numbers, in which case the light reflected by the newly-formed water or vapour, might present the appearance of luminous bands): but if such an ignition and explosion were to take place in lower strata, it might give rise to the showers of rain which fall so suddenly in water-spouts. The atmosphere, however, contains a large proportion of nitrogen, and in its lower strata, carbonic acid, as also various foreign substances, more especially in the vicinity of volcanos, (where, as we shall see in the sequel, these meteors are of very frequent occurrence,) carried up into it by evaporation and other means. And should a combination with such substances take place, red rain, meteoric dust, and perhaps even aerolites might be formed. The ignition of such foreign matter might also account for the various hues of different shooting stars, and also for the trains of sparks occasionally observed.

If we can suppose this hypothesis to be an approximation to the truth, it would unfold to us highly interesting views of processes continually in progress in nature's laboratory:—the perpetual formation and decomposition of water; dissolution and renovation unceasingly occurring in the very constitution of that portion of the terrestrial globe, which, among all the geological revolutions that have taken place on its surface, appears to have suffered the least change—the world of waters*; changes analogous to those observable in all other departments of the natural world; changes by which, in fact, Nature appears to maintain "her health, her beauty, her fertility." And these remarkable meteors might thus be regarded as celestial beacons, shining forth to impart to us information of physiological changes in the very act of occurring, perhaps, in the remotest regions of our atmosphere.

Should such be the origin and office of these meteors, we might, however, expect to find them of continual occurrence,

* The decomposition and formation of water is continually in progress on the earth's surface by combustion and other means; but Nature plans for a globe uninhabited by man; and, with the exception of rare and occasional conflagrations caused by the electric fluid, and perhaps by spontaneous ignition, fire is not kindled on the earth without human agency.

rather than making their appearance as periodical visitors. And, in fact, although they doubtless seem to have their gala nights, M. Quetelet infers, from observations he has made, that in our latitudes about sixteen shooting stars of various sizes may be seen on a dark night in the course of an hour. Nor need we suppose the phænomena to be discontinued during the hours of sun-light. And in the warmer regions of the globe, the phænomenon of shooting stars is, according to M. de Humboldt, of very frequent occurrence. Thus, when off the coast of Africa, and especially near the Canaries, that eminent traveller speaks of "the innumerable multitude of falling stars, which appeared every instant." He adds, "That he never saw them so multiplied as in the vicinity of the volcanos of Quito, and in the part of the Pacific Ocean which bathes the volcanic coasts of Guatemala." He also describes these meteors as being very brilliant, and as usually either leaving trains behind them, which remain luminous twelve or fifteen seconds, or bursting into sparks. And, should the great height above the earth's surface assigned to some of these meteors* seem to militate against the hypothesis here advanced, on the other hand, it cannot but appear that the fact of their frequent occurrence in volcanic regions distinctly leads to the inference that they are not solely cosmical phænomena, but absolutely and intimately connected with the physical constitution of our own planet. And when we consider the vast number of these meteors†, we cannot but feel convinced that they are not mere accidental displays, calculated to alarm the ignorant, and to excite the wonder and curiosity of the learned, but that they actually perform some important though hitherto unperceived office in the œconomy of nature.

I am well aware that the above attempt at explanation does not account for the periodicity of these meteors, which, in fact, constitutes one of their most remarkable features; but perhaps we can scarcely expect to arrive at any satisfactory conclusion as to the cause of their periodicity, until we become in some measure acquainted with their nature and origin. The facts above adduced prove that the periodical shooting stars are not isolated meteorological phænomena; whilst their evident connexion with the aurora borealis leads us to regard

* One hundred German miles (466 English miles) or more.—See an article by M. Quetelet from the *Annuaire de Bruxelles*, in Phil. Mag. for September 1837. [Third Series, vol. xi. p. 271.]

† At the rate of 16 per hour, according to M. Quetelet's observations, not less than 140,000 annually; or, if this be considered as too large a proportion, let us take only half that number, and this in a comparatively circumscribed space, and in a temperate parallel of latitude.

them as of electric origin. If, then, we consider these phænomena as originating in electricity in motion; and if, to use the words of Sir John Herschel, we consider "that the sun's rays are the ultimate source of every motion that takes place on the surface of the earth," (possibly modified not only by the direction in which they strike the earth in its transit through space, but also by the varying rapidity with which the earth itself moves in different parts of its orbit,) we may perhaps regard the sun as mainly influential in their production. And therefore, although meteorological observations must necessarily be of great importance as tending to elucidate the subject*, it cannot but be hoped that astronomy will also lend its powerful aid, and that future observations made on the sun's disc, at, or immediately antecedent to the time indicated by the periodical appearance of these meteors, and compared with the ordinary phænomena presented by its surface, (both in our latitudes and in tropical regions, where shooting stars are of more common occurrence,) may detect some unusual agitation, some apparently peculiar condition in the sun itself at those especial periods, to which may be attributed the periodical occurrence of these meteoric phænomena.

Clapham, Nov. 6, 1840. ROSINA MARIA ZORNLIN.

An accidental circumstance has retarded the publication of the above paper until the present period; and, in the interim, two rather important articles have been given to the public on the subject of these meteors; the one forming a portion of Professor Forbes's 'Supplementary Report on Meteorology,' given in the 'Report of the Tenth Meeting of the British Association for the Advancement of Science;' and the other consisting of a paper read before the Royal Astronomical Society, and published in the Monthly Notices of that Society. Although nothing occurs in either of these articles which I had not previously taken into consideration, yet, as I may appear in the former portion of this paper to have passed over some points with too slight a notice, I gladly avail myself of the opportunity thus afforded me of adding a few additional remarks.

The periodicity of the November and August meteors I have considered as granted: Professor Forbes does not appear to regard this point as established; but observes, "that we

* The apparently periodical recurrence of the aurora borealis, which, as observed by Captain Beechey near Behring's Straits, made its first appearance for two successive years, 1831 and 1832, on the 26th of August, is not undeserving of notice. A splendid aurora was observed in this country by Mr. S. Hunter Christie on the 25th of August, 1837.—See Seventh Report of the British Association for the Advancement of Science.

must use the term periodicity, as applied to these meteors, with caution*." That philosopher, however, does not seem to regard the aurora borealis as connected with, and occasionally appearing as a substitute for the phænomena of the shooting stars; the fact, however, is attested by his own observations in 1837, and by those of others in 1838. He further remarks, that no marked indication was observed in 1839 and 1840. At this place, in both those years, the nights of November 12th were most unfavourable for observation; that of November 12th, 1839, having been uniformly foggy; and that of November 12th, 1840, hazy and dull in the early part of the evening, and subsequently rainy. In 1836, the weather at this epoch was not more propitious; and in 1837, except in the early part of the evening (when the phænomenon before described was witnessed), the sky was overcast throughout the night, the moon being also at the full. On the 10th of August of the present year (1841) we had here a dull evening and a rainy night: on the 9th, however, the phænomenon of shooting stars was witnessed in far more than usual abundance; and among those I observed, although the larger proportion were direct, or moving from N.E. to S.W., three were retrograde, or moving from S.W. to N.E.; and the latter included a splendid meteor, much larger than Venus, which burst into sparks.

The predominating direction of these meteors is another point to which, perhaps, some allusion ought to be made. The hypothesis of Chladni, and that which has been most generally adopted, consists, as stated by Mr. Galloway, "in supposing that, independently of the great planets, there exist in the planetary regions myriads of small bodies which circulate about the sun, generally in groups or zones, and that one of these zones intersects the ecliptic about the place through which the earth passes in November." Besides other difficulties attending this hypothesis, (none of which, it will be found, apply to the hints at explanation I have offered above,) Mr. Galloway remarks, "That bodies moving in groups in the circumstances supposed, must necessarily move in the same direction, and consequently, when they become visible from the earth, would all appear to emanate from one point and move towards the opposite. Now, although the observations seem to show that the predominating direction is from north-east to south-west, yet shooting stars are observed on the same nights to emanate from all points of the heavens, and to move in all possible directions." This difficulty, it will be

* Report of the British Association for the Advancement of Science, 1840, p. 119.

evident, would not apply to the hypothesis I have ventured to adduce; and, indeed, if these meteors might be regarded as owing their origin to energetic electric currents, it would not be difficult to assign a supposititious reason for the general, although not invariable direction of these meteors contrary to the motion of the earth in its orbit, and also for their greater frequency in tropical regions. This, however, would embrace too wide a field to enter upon at present.

Since the former part of this paper was written, M. Quetelet's memoir, containing his *Catalogue des Principales Apparitions des Etoiles filantes* (which I had not previously seen), has been placed in my hands. From this it appears, that M. Quetelet, as well as other distinguished observers, concur with me in the opinion that the periodical shooting stars are connected with other meteorological phænomena. I also perceive that M. Quetelet cites some of the same instances adduced by me in support of this assumption. The additional facts given in my paper may, however, prove both interesting and confirmatory.

In addition to the periodical appearance of these meteoric phænomena on the 12th of November and the 10th of August, M. Quetelet has pointed out other epochs when similar meteoric phænomena have been observed, recurring with apparent periodicity. The epochs thus indicated are the 2nd of January; the 23rd or 24th of April; from the 15th to the 20th of June; the 18th of October; and the 6th or 7th of December. The increased number of the apparently periodical meteoric phænomena perhaps adds to the difficulties attending the hypothesis of a zone or zones intersecting the ecliptic. To a hypothesis attributing their origin to electric currents, the more frequent recurrence of such phænomena offers no difficulty whatever: for, if electric currents be occasionally thus energetically developed, there appears no reason to infer that this phænomenon may not repeatedly occur, whether periodically or otherwise, and not be restricted to one or two annual displays.

October 6, 1841. R. M. Z.

L. *An Analysis of the Atmosphere of some of the Cornish Mines. By* M. P. Moyle, *Esq.**

A PREMIUM having been offered for "the best analysis of the air taken at the termination of a *core* of two men, from the extremity of one mine level in granite, and of another in killas; the samples to be as fairly taken as possible, and in

* Communicated by the Author.

measure not less than one gallon each : it is desirable that the ends should be at least fifteen fathoms from any shaft or winse, not more than twenty fathoms above the deepest level of the mine, nor less than 100 fathoms below the adit, &c.," through the Royal Cornwall Polytechnic Society, and that premium having been awarded to the author of this communication, he has considered that an abridged extract from the essays published by that Society in their Transactions for 1839 and 1840, may be acceptable to the readers of the Philosophical Magazine.

The analysis of gases, and more especially their combination or mixture in atmospheric air, being attended with no small difficulty of manipulation, (nor are the precautions requisite for obtaining accurate results of no mean consequence) it may be desirable to premise with a few observations on the different points of consideration so necessary for this purpose, and which are fully detailed in the essays themselves.

A due correction was made in all the following experiments, with the utmost care, for any change of temperature during the progress of the individual experiment.

The necessary allowance for the presence of vapour in the gas was always made according to Dr. Dalton's table. The unaccountable tendency or propensity which gases possess to unite, by overcoming obstacles placed for their security or separation, was never lost sight of; and no experiment was recorded which suffered sufficient interruption to allow of the above circumstance to transpire. In addition to this, the samples of air, although they gave perfect satisfaction as to the security of the vessels in which it was contained, were operated on with little or no loss of time. The pressure and temperature of the atmosphere were always reduced to one common standard, viz. barometer 30 inches, and thermometer 60°; and whenever the lower surface of the air in the jar did not coincide with the exact level of the surface of the liquid in the trough, the density of the included gas was reduced to that of the external atmosphere.

The utmost care was had in the manufacture of the oxygen, hydrogen, nitrous gas, &c. &c., that they might be of the utmost purity. The oxygen was uniformly made from the chlorate of potash in a green glass retort; and although the zinc of commerce was used for the manufacture of the hydrogen, it was invariably purified by passing it immediately before use through a solution of pure potash, for the purpose of removing any carburetted hydrogen or other impurity, and depriving it of any oxygen which it might contain, by subjecting it to the influence of the spongy platina ball, heated

to redness; and sulphuretted hydrogen may be removed by the gas being afterwards passed through a solution of chlorate of lime. The nitrous gas or deutoxide of nitrogen was procured by the action of nitric acid in pure metallic copper, collecting the gas sometimes over water, and sometimes over mercury.

The quadrisulphuret of lime was manufactured after the manner recommended by Dr. Dalton.

The samples of air taken from the different loads in the mines were procured by emptying jars of water of their contents, and four-ounce bottles of mercury from one to the other, at the required spots, preserving a small portion of the water or mercury, greasing the corks, driving them as tight as possible, and afterwards sealing them over. Both jars and bottles were kept in an inverted position from this moment to that of their use; and a further proof of their security was generally had by an escape of a few bubbles of air on withdrawing the cork or stopper under water in the pneumatic trough.

The pneumatic trough was filled with perfectly clean rain water, collected from the glass roof of a green-house, and renewed as frequently as opportunities would permit.

Sample 1. One gallon.

Wheal Vor Mine.—Tin in slate. Taken from the 250-fathom level below the surface; fifteen fathoms west of any shaft or winse, and taken a few minutes after firing a hole, with two men in a core; specific gravity of air ·997.

	Oxygen.
Average of three experiments by exposing the gas to the influence of the quadrisulphuret of lime in Hope's eudiometer	18·41
Average of four experiments with pure hydrogen fired by electricity in Volta's eudiometer . . .	18·40
Average of four experiments with nitrous gas . .	18·45
One experiment with heated platina ball over mercury	18·40
Mean . . .	18·416

	Carb. acid.
Average of three experiments with lime-water . .	0·06
Average of three experiments with solution of pure potash	0·07
Mean . . .	00·65

No trace of any other gas, consequently the composition of this sample stands thus :—

Nitrogen	81·519	per cent.
Oxygen	18·416	...
Carbonic acid	0·065	...
	100·000	

Sample 2. One gallon.

Wheal Vor.—As before. 250 fathoms from surface; sixteen fathoms east of any shaft or winse; two men in a core; ten minutes after firing a hole; specific gravity ·993.

	Oxygen.
Average of six experiments with hydrogen	16·7
Average of four experiments with nitrous gas	16·62
Average of two experiments with platina ball	16·75
Mean . . .	16·69

	Carb. acid.
Average of three experiments with lime water . . .	0·075
Average of three experiments with alkaline solution	0·075
Mean . . .	0·075

Composition of this sample:—

Nitrogen	83·24	per cent.
Oxygen	16·69	...
Carbon. acid	0·075	...
	100·005	

Sample 3. One gallon.

Wheal Vor.—As before. 240 fathoms; twenty-four fathoms east of any shaft or winse; two men in a core; taken half an hour after firing a hole; specific gravity ·997.

	Oxygen.
Average of three experiments with hydrogen	19·2
Average of three experiments with nitrous gas	19·0
Average of two experiments by the slow combustion of phosphorus over mercury	18·6
One experiment with platina ball	19·0
Mean . . .	18·95

	Carb. acid.
Average of three experiments with lime water . . .	0·06
Average of three experiments with alkaline solution	0·07
Mean . . .	0·065

A sample of the above air taken in a four-ounce bottle secured by mercury, gave strong indications of both sulphuretted hydrogen and sulphurous acid: quantity not ascertained.

Composition of this sample:—

Nitrogen	. . . . .	80·98 per cent.
Oxygen	. . . . . .	18·95 ...
Carbonic acid	. .	0·065 ...
		99·995

Sample 4. One gallon.

Wheal Vor.—As before. 230 fathoms; two men in a core; 22 fathoms from shaft or winse, half an hour after firing a hole; specific gravity ·994.

	Oxygen.
Average of two experiments with hydrogen	17·22
Average of three experiments with nitrous gas . . .	17·36
Average of three experiments with quadrisulphuret of lime	17·30
Average of three experiments with hydrogen on sample taken from bottle secured by mercury, as before	17·20
Average of two experiments from ditto with quadrisulphuret of lime	17·33
Mean . . .	17·282

	Carb. acid.
Average of two experiments with lime water	0·08
Average of two experiments with alkaline solution . .	0·085
Mean . . .	0·082

	Sulph. hydrogen.
Average of four experiments with a cup of nitric acid standing over mercury, with sample of air taken in a mercurial bottle	0·08 p. ct.

Also, a strong trace of sulphurous acid; therefore composition of this sample,

Nitrogen	. . . .	82·556 per cent.
Oxygen	. . . .	17·282 ...
Carbonic acid	. . . . . .	0·082 ...
Sulphuretted hydrogen	. .	0·080 ...
Sulphurous acid	. . . . .	strong trace
		100·000

Sample 6. A quart; also mercurial sample.

Wheal Vor, Penhale.—Slate. 36-fathom level; four men at work in a dead end; candle burned with difficulty; specific gravity 0·994.

	Oxygen.
Average of two experiments with hydrogen	14·74
Average of two experiments with nitrous gas	14·77

	Oxygen.
Average of two experiments with hydrogen from mercurial sample	14·77
Average of two experiments with nitrous gas from do.	14·77
Mean . . .	14·76

	Carb. acid.
Average of two experiments with potash solution . . .	0·23
Average of two experiments with potash solution from mercurial sample	0·23
Mean . . .	0·23

A trace of both sulphuretted hydrogen and sulphurous acid in both samples.

Composition of sample:—

Nitrogen	85·01	per cent.
Oxygen	14·76	...
Carbonic acid . .	0·23	...
	100·00	

Sample 7. One gallon.

Great Work.—Granite, tin and copper. 170 fathoms from surface; thirty fathoms from winse or shaft, with two men in a core; taken directly after firing a hole; specific gravity ·991.

	Oxygen.
Average of four experiments with hydrogen	16·99
Average of three experiments with nitrous gas	17·00
Average of three experiments with quadrisulphuret of lime	17·05
Experiment with platina ball	17·00
Mean . . .	17·01

	Carb. acid.
Average of three experiments with lime water	0·09
Average of two experiments with potass solution . . .	0·10
Mean . . .	0·095

Composition of sample:—

Nitrogen	82·895	per cent.
Oxygen	17·010	...
Carbonic acid . .	0·095	...
	100·000	

Sample 8. One gallon.

Great Work.—As before. 160 fathoms from surface; two men in a core; thirty fathoms from winse or shaft; fifteen minutes after firing a hole; specific gravity ·992.

	Oxygen.
Average of two experiments with hydrogen	15·1
Average of three experiments with nitrous gas . . .	15·2
Average of three experiments with quadrisulphuret of lime	15·15
Mean . . .	15·15

	Carb. acid.
Average of two experiments with lime water	0·14
Average of two experiments with solution of potash	0·15
Mean . . .	0·145

Composition of sample :—

Nitrogen	84·705	per cent.
Oxygen	15·15	...
Carbonic acid . . .	0·145	...
	100·000	

Sample 9. One quart.

Great Work.—As before. 150 fathoms from surface; two men in a core; twenty fathoms from winse or shaft, directly after firing a hole; specific gravity ·995.

	Oxygen.
Average of two experiments with hydrogen	16·30
Average of two experiments with nitrous gas . . .	16·40
Average of two experiments with quadrisulphuret of lime	16·47
Average of two experiments with hydrogen from mercurial sample	16·48
Average of two experiments with quadrisulphuret of lime from ditto	16·60
Mean . . .	16·45

	Carb. acid.
Average of two experiments with lime water	0·10
Average of two experiments with potash solution . .	00·98
Mean . . .	0·099

Composition of sample :—

Nitrogen	83·451	per cent.
Oxygen	16·45	...
Carbonic acid	0·099	...
	100·000	

Sample 10. One quart.

Great Work.—As before. 125 fathoms from surface; twenty-

three fathoms from winse or shaft; two men in a core; half an hour after firing a hole; specific gravity 0·998.

	Oxygen.
Average of three experiments with nitrous gas . . .	17·56
Average of three experiments with hydrogen . . .	17·50
Average of three experiments with quadrisulphuret of lime	17·56
Average of two experiments with hydrogen in mercurial trough from a mercurial sample	17·54
Average of two experiments with nitrous gas in ditto from ditto	17·56
Mean . . .	17·544

	Carb. acid.
Average of three experiments with lime water . . .	0·10
Average of two experiments with potash solution . .	0·10
Mean . . .	0·10

A trace of both sulphuretted hydrogen and sulphurous acid was observable in the mercurial samples.

Composition of sample:—

Nitrogen	82·356	per cent.
Oxygen	17·544	...
Carbonic acid . . .	0·100	...
	100·000	

Sample 11. One gallon.

Binner Downs.—Copper mine; slate. 104-fathom level; eighteen fathoms from winse or shaft; two men in a core in a rise; specific gravity 0·996.

	Oxygen.
Average of four experiments with hydrogen	16·73
Average of two experiments with nitrous gas	16·80
Average of two experiments with quadrisulphuret of lime	16·77
Average of three experiments with hydrogen in mercurial trough from a mercurial bottle sample	16·76
Average of two experiments with nitrous gas in ditto from ditto	16·76
Mean . . .	16·764

	Carb. acid.
Average of five experiments with lime water and solution of potash	0·09

A strong trace of sulphuretted hydrogen in the mercurial sample. Composition of sample, therefore,

Nitrogen	83·146 per cent.
Oxygen	16·764 ...
Carbonic acid	0·090 ...
Sulphuretted hydrogen . . .	strong trace.
	100·000

Sample 12. One gallon.

Carn Brea.—Copper mine, granite. Immediately after firing a hole; from the 105-fathom level from surface, and twenty-five fathoms from any winse or shaft; two men in a core; specific gravity ·994.

	Oxygen.
Average of two experiments with hydrogen	16·72
Average of three experiments with nitrous gas . . .	16·66
Average of three experiments with quadrisulphuret of lime	16·70
Mean . . .	16·693

	Carb. acid.
Average of four experiments with lime water and solution of potash	0·07

Composition of sample:—

Nitrogen	83·237 per cent.
Oxygen	16·693 ...
Carbonic acid . . .	0·070 ...
	100·000

Sample 13. One gallon.

Carn Brea.—As before. 95-fathom level, machine blowing air; two men in a core; spot wrought only sixteen hours out of the twenty-four; sample taken three quarters of an hour after firing a hole; specific gravity ·992.

	Oxygen.
Average of three experiments with hydrogen	14·50
Average of two experiments with nitrous gas	14·64
Average of three experiments with quadrisulphuret of lime	14·40
Mean . . .	14·51

	Carb. acid.
Average of three experiments with lime water	0·12
Average of three experiments with potash solution . .	0·14
Mean . . .	0·13

Composition of sample:—

Nitrogen	85·36	per cent.
Oxygen	14·51	...
Carbonic acid . .	0·13	...
	100·00	

Sample 14. One quart.

Tresavean.—Copper mine, in granite. 156-fathom level; 208 fathoms from surface; two men in a core; directly after firing a hole; sixty-five fathoms from winse or shaft; specific gravity ·993.

	Oxygen.
Average of three experiments with hydrogen . . .	16·20
Average of two experiments with nitrous gas . .	16·50
Mean . . .	16·35

	Carb. acid.
Average of two experiments with lime water	0·13
Average of two experiments with potash solution . .	0·13
Mean . . .	0·13

A slight trace of sulphuretted hydrogen.

Composition of sample:—

Nitrogen	83·52	per cent.
Oxygen	16·35	...
Carbonic acid . . .	0·13	...
	100·00	

Sample 15. One gallon.

Wheal Ann.—Tin mine, in granite. A rise in the 80-fathom level; sixteen fathoms from any shaft or winse; two men in a core; no gunpowder used; specific gravity of air ·995.

	Oxygen.
Average of six experiments with hydrogen and quadrisulphuret of lime	16·72

	Carb. acid.
Average of four experiments with lime water and solution of potash	0·08

Composition of sample:—

Nitrogen	83·20	per cent.
Oxygen	16·72	...
Carbonic acid . . .	0·08	...
	100·00	

Sample 16. One gallon.

Wheal Ann.—As before. 100-fathom level; forty-five fa-

thoms from winse or shaft; machine and water-fall forcing air; end of a core of two men; no gunpowder used for some time; specific gravity ·997

	Oxygen.
Average of seven experiments with hydrogen, nitrous gas and quadrisulphuret of lime	18·22
	Carb. acid.
Average of five experiments with lime water	0·07

Composition of sample:—

Nitrogen	81·71	per cent.
Oxygen	18·22	...
Carbonic acid . . .	0·07	...
	100·00	

Sample of the above taken in a mercurial bottle gave the following means:—

Oxygen	18·34	per cent.
Carbonic acid . . .	0·07	...

Sample 17. Four gallons.

Consols.—Copper mine, in slate. No other report forwarded but that they were taken from the bottom levels. The proper directions having been forwarded for the taking of the samples, it is presumed the necessary precautions were adopted as they came to hand in an inverted position, &c. &c.; specific gravity ·997.

	Oxygen.
Average of four experiments with hydrogen	17·93
Average of two experiments with nitrous gas	17·82
Average of three experiments with quadrisulphuret of lime	17·88
Average of two experiments with phosphorus	17·60
Average of two experiments with platina ball	17·70
Mean . . .	17·78
	Carb. acid.
Average of three experiments with lime water	0·06
Average of three experiments with potash	0·06
Mean . . .	0·06

Composition of sample:—

Nitrogen	82·16	per cent.
Oxygen	17·78	...
Carbonic acid . . .	0·06	...
	100·00	

Sample 18. Four gallons.

Consols.—As before. Sample obtained under similar circumstances to the last; specific gravity ·997.

	Oxygen.
Average of three experiments with hydrogen . . .	18·41
Average of three experiments with quadrisulphuret of lime	18·41
Average of two experiments with nitrous gas . . .	18·45
Mean . . .	18·425
	Carb. acid.
Average of three experiments with lime water . . .	0·06
Average of two experiments with potash solution . .	0·07
Mean . . .	0·065

Composition of sample :—

Nitrogen	81·52	per cent.
Oxygen	18·42	...
Carbonic acid . . .	0·06	...
	100·00	

Summary of foregoing experiments:—

Average quantity of oxygen per cent.	17·067
Average quantity of carbonic acid	0·085
Average quantity of nitrogen	82·848
	100·000

The results of the foregoing experiments clearly demonstrate the impure state of the atmosphere which a miner is compelled to breathe for about one-third of the period of his existence (generally eight hours at a time out of the twenty-four). There cannot be a doubt, that where much gunpowder is used often pernicious gases are generated, the detection of which will not be easy in samples taken with water, as I have failed in almost every instance in finding a trace of such, by the nicest tests. That sulphurous acid is produced, there cannot be question, as it may be found invariably in the water preserved in the jars, when the samples have been taken immediately after the explosion of a hole, as well as in the air well preserved in a bottle by mercury, and very speedily experimented on in a mercurial trough.

The minutiæ of preparing the different tests, and the precautions adopted in the foregoing experiments, are fully detailed in the above-mentioned Transactions, and it is therefore considered unnecessary to recapitulate them here.

LI. *Remark on Primitive Radices. By the* Rev. R. Murphy*.

HAVING had the honour of receiving from M. Jacobi his 'Canon Arithmeticus, sive tabulæ, quibus exhibentur pro singulis numeris primis, vel primorum potestatibus infra 1000, numeri ad datos indices et indices ad datos numeros pertinentes, Berolini, 1839,' I sought in the tables for some law relative to primitive radices. Being desirous of completing a work, entitled 'New Forms for the calculation of Logarithms,' which is nearly ready for the press, I could not bestow sufficient time to investigate the absolute generality of the following properties; yet even as empiric forms easily recollected, and verified in every case in those tables, and also in every case where I have taken small primitive radices, they will be found useful; at the same time the attention of mathematicians will, I hope, be drawn to the demonstration of the generality of these forms (or the contrary), though belonging to what is usually considered a dry subject, "The Theory of Numbers."

In page 6, we find a table, "Numeri primi, quorum 10 radix primitiva est," viz. 7, 17, 19, 23, 29, 47, 59, &c. to 2543; and the table from which this is deduced is (with some exceptions) carried as far as 9901. The following prime numbers, to which 10 is primitive radix, may be *culled* from those in Burckhardt's table, viz. 7, 17, 47, 97, 167, 257, 367 (497 is not prime), 647 (817 is not prime), (1007 is not prime), 1217, 1447, 1697 (1967 is not prime). Hence every prime number of the form $10n^2 + 7$ has 10 for a primitive radix, as far as can be verified by these tables.

As this property, if general, must depend on some relation between the numbers 7 and 10, I remarked that 7 was the only prime number less than 10, to which the latter was the primitive radix; accordingly, I tried in several instances the more general formula $an^2 + p$, where p is a prime number less than a number, a to which a is a primitive radix (as $3n^2 + 2$, $5n^2 + 3$, &c.); from the results it would appear to be a general property, that under those circumstances $an^2 + p$ $\left(\text{when prime, and } p \text{ greater than } \frac{a}{2}\right)$ will have a as a primitive radix. From the cause I have mentioned, I have not bestowed that attention to these properties I could have wished; the attention of others will now be called to them; in the mean time they may stand as empiric formulæ.

London, October 1841. Robert Murphy.

* Communicated by the Author.

LII. *On the Perchlorate of the Oxide of Æthule, or Perchloric Æther.* By CLARK HARE, *and* MARTIN H. BOYE*.

THE energetic properties of perchloric acid, and its stability, compared with the other compounds of chlorine with oxygen, led us to the belief that this acid might be combined with the substance which performs the part of a base in that class of organic salts which are generally designated by the name of *æthers*, and for which Berzelius, in consequence of his theoretical views, has adopted the name of oxide of æthule. For this purpose, a concentrated solution of perchlorate and sulphovinate of barytes, in equivalent proportions, was subjected to distillation. The sulphovinate of barytes may be considered as a double sulphate of barytes and the oxide of æthule; and we anticipated that, when heat was applied, a double decomposition would take place between the latter and the perchlorate of barytes. So long as the salts remained in solution no reaction occurred; but as soon as they became solid, in consequence of the distillation of the water, a reciprocal decomposition ensued, and a sweet æthereal liquid distilled into the receiver. *This liquid* is the *perchlorate of the oxide of æthule.*

As this substance is extremely explosive, in order to prepare it with safety it is necessary to operate on small quantities. We have employed from seventy to ninety grains of crystallized sulphovinate of barytes, with an equivalent proportion of perchlorate of barytes †; but we would recommend, especially on the first performance of the experiment, the employment of considerably smaller quantities. The salts should be intimately mixed in a mortar, and placed in a small retort attached to a refrigerator containing ice, and a receiver similarly cooled. The retort is to be heated in an oil-bath, in which a thermometer is suspended, so as to indicate the temperature. A wooden screen, furnished with openings covered with thick plate-glass at such intervals as to afford a full view of the different parts of the apparatus, should be erected in front of it, and strings passed round the screen and at-

* From the Transactions of the American Philosophical Society, vol. viii.; having been read before the Society, December 4th, 1840.

† The amount of barytes in the perchlorate should be ascertained by an experiment, as it retains water with great tenacity. It may be worth while to mention, that the perchlorate of potassa cannot be substituted for the perchlorate of barytes, since the sulphovinate is decomposed without acting on it. We were equally unsuccessful in an attempt to procure the æther by the distillation of perchlorate of barytes and concentrated sulphovinic acid.

tached to a bar traversing on a pivot, and supporting an Argand spirit-lamp, by which heat is communicated to the oil-bath, so as to enable the flame of the lamp to be removed from or applied to the apparatus, according to the indications of the thermometer, without exposing the person of the operator. After the heat has reached 212° Fahr., below which the salts employed do not react on each other, it should be raised very gradually, and the distillation finished below 340° Fahr. Under these circumstances but little danger is to be apprehended from the retort; but the æther in the receiver must be treated with the greatest caution, since it has exploded in our hands in attempting to remove it with a pipette from the stratum of water which covers it. This water, therefore, should be removed by the cautious use of strips of blotting-paper, moistened at the end, and introduced into the tube employed as a receiver.

To avoid the danger attendant on the management of the æther in its pure state, it may be received in strong alcohol, since it is not explosive when dissolved in alcohol. If the experiment be performed with seventy grains of sulphate of barytes, from one to two drachms of absolute alcohol will be found sufficient for this purpose. By the addition of an equal volume of water, the æther may subsequently be separated from the solution in small quantities for the purpose of examination; but in this case a loss of æther is sustained by the decomposing influence of the water employed.

The perchlorate of æthule obtained in this way is a transparent, colourless liquid, possessing a peculiar, though agreeable smell, and a very sweet taste, which, on subsiding, leaves a biting impression on the tongue resembling that of the oil of cinnamon. It is heavier than water, through which it rapidly sinks. It explodes by ignition, friction, or percussion, and sometimes without any assignable cause. Its explosive properties may be shown, with but little danger, by pouring a small portion of the alcoholic solution into a small porcelain capsule, and adding an equal volume of water. The æther will collect in a drop at the bottom, and may be subsequently separated by pouring off the greater part of the water, and throwing the rest on a moistened filter, supported by a wire. After the water has drained off, the drop of æther remaining at the bottom of the filter may be exploded either by approaching it to an ignited body, or by the blow of a hammer. We are induced to believe, that in explosive violence it is not surpassed by any substance known in chemistry. By the explosion of the smallest drop, an open porcelain plate will be broken into fragments, and by that of a larger quantity, be

reduced to powder. In consequence of the force with which it projects the minute fragments of any containing vessel in which it explodes, it is necessary that the operator should wear gloves, and a close mask, furnished with thick glass plates at the apertures for the eyes, and perform his manipulations with the intervention of a moveable wooden screen*.

In common with other æthers, the perchlorate of æthule is insoluble in water, but soluble in alcohol; and its solution in the latter, when sufficiently dilute, burns entirely away without explosion. It may be kept for a length of time unchanged even when in contact with water: but the addition of this fluid, when employed to precipitate it from its alcoholic solution, causes it partially to be decomposed. Potassa, dissolved in alcohol, and added to the alcoholic solution, produces immediately an abundant precipitate of the perchlorate of that base; and, when added in sufficient quantity, decomposes the æther entirely. It would appear, therefore, impracticable to form either perchlorovinates or perchlorovinic acid.

We have subjected the perchlorate of æthule to the heat of boiling water with explosion or ebullition.

It may be observed, that this is the first æther formed by the combination of an inorganic acid containing more than three atoms of oxygen with the oxide of æthule, and that the chlorine and oxygen in the whole compound are just sufficient to form chlorohydric acid, water, and carbonic oxide with the hydrogen and carbon.

The existence of a compound of the oxide of æthule with an acid containing *seven* atoms of oxygen, led us to attempt to combine, by the same method, this base with nitric acid. For this purpose we subjected a mixture of sulphovinate and nitrate of barytes to the same treatment as described above; but the reaction, even when conducted with the greatest possible care, is destructive, hyponitrous æther and gaseous matters being the principal products obtained. Nor were we more successful in our attempts to procure a sulphurous or hyposulphuric æther by the same process.

LIII. *Note on the Theory of Light. By* Professor Powell†.

IN my recently published work 'On the Undulatory Theory as applied to the Dispersion of Light,' &c., I have given abstracts of the various investigations related to the main sub-

* Having suffered severely, on several occasions, from the unexpected explosion of this substance, we would earnestly recommend the operator not to neglect the precautions mentioned above.

† Communicated by the Author.

ject which have been pursued by several mathematicians, and especially by M. Cauchy, so far as they have come to my knowledge; so various, however, are the channels through which such researches are made public, that some of them, especially those which appear on the Continent, often escape attention. In this way I now find I have omitted to notice some of the later investigations of the distinguished mathematician just named.

M. Cauchy has favoured me with a letter, in which, acknowledging the receipt of a copy of my work, he adds a brief mention of these later investigations, to which I had not referred in my volume, with a request that I would take an opportunity of supplying my readers with this further information. As it may be *some time* before a work on abstract science reaches a second edition, perhaps the best course I can pursue is to offer a very brief statement of the points in question through the medium of this Journal.

It appears then, that subsequently to the researches of which I have given abstracts, M. Cauchy published in August 1836, at Budweiss, a lithographed memoir entitled *Mémoire sur la Théorie de la Lumière*, &c., which bears much on the subject of the general theory, and the phænomena to which it relates, as considered in my tract.

The main heads of this investigation are as follows:—

1. The general equations of motion of the æther: 2nd. Colour, or the dispersion: 3rd. The motion of light penetrating to a small depth into the interior of opake bodies: 4th. The transition from the formulæ obtained under the third head, to those which represent vibratory motion in general in an æthereal fluid: 5th and 6th. The case of media in which the propagation of light takes place in the same manner in every direction, whether round any point, or round every axis parallel to a given straight line: 7th. The propagation of plane waves in transparent bodies.

The consequences have been further followed out by the author in other memoirs published since.

In the second volume of the *Comptes Rendus*, 1836 (1st semestre), p. 365, M. Cauchy has observed that the intensity of the light penetrating to a small depth x in the interior of an opake body, decreases in proportion to a negative exponential of the form ε^{-cx}.

Exponentials of the same kind are found entering into many of the formulæ included in the memoir of August 1836; and setting out from these formulæ, he shows (p. 84 of that memoir) how in a coloured glass the thickness necessary to produce the extinction of a luminous ray may vary with the nature of the colour.

Among the formulæ of § 7. we may distinguish the equations (44.) (9.) and (51.), from which follow (as the author observes, p. 95) the laws of rectilinear, circular and elliptic polarization; and it may be remarked that the equation (9.), relative to a transparent medium, viz.

$$\xi = A\cos(kz - st + \lambda) \quad \eta = B\cos(kz - st + \mu)$$
$$\zeta = C\cos(kz - st + \nu)$$

is deduced from the general equations which represent the infinitely small motions of a system of molecules that is from the equations (16.) of the memoir on Dispersion, for obtaining which the author has made no particular assumption, and has no occasion to make any term vanish. He also observes (p. 83) that we cannot in general reduce the equations (9.) to those which represent rectilinear polarization, and in which λ, μ, ν are equal.

The equations (16.) of the memoir on the Dispersion are the differential equations of motion in their most general form, corresponding to those of art. 59. in my tract.

The reader will immediately perceive the relation which these researches bear to the general subject. These last results accord exactly with mine obtained by a different method, as well as with those of the late Mr. Tovey, by whose recent death the science of this country has sustained so great a loss.

Oxford, Oct. 3, 1841.

LIV. *Results of some Experiments in Electricity and Magnetism.* By William Petrie, *Esq.**

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

BEING desirous of obtaining certain information relating to electricity and magnetism, which I failed to find in various works on these subjects at the time published, I was induced to make experiments, from which the following are the concise statements, showing the results, and which may, perhaps, possess sufficient interest to find a place in your Journal.

It is commonly stated, as by Lenz, that the conducting power of wires varies as the section, and inversely as the length; but this only refers to a particular case, namely, when the amount of electricity which the battery can circulate when the current is quite unimpeded (such as when the circuit is made with a very short thick wire), is very great com-

* Communicated by the Author.

pared with what does pass, on account of the great length and small section of the wire experimented with.

Adopting, for brevity, the following notation :—

L = length of wire.

S = sectional area of wire.

n = a multiple, or simply a number.

Q = quantity of electricity which the battery can circulate, when the passage of the current is quite unimpeded; that is, the absolute power of the battery.

q = that portion of Q, which passes through the wire experimented with, when it is included in the circuit.

e = a constant, while L and S are constant; being 1, when such a measure is taken for Q, that, on Q being reduced to 1, $q = \frac{Q}{2}$.

My object has been to obtain the law of conduction, giving the values of q for all values of Q as well as of L and S.

In a wire of constant L and S, by varying Q, I find

$$q = \frac{e \, . \, Q}{e + Q} \quad \ldots\ldots\ldots\ldots \quad \text{(formula A).}$$

The quantity (q) of electricity which (from a given Q) will pass through a length L, may be considered as a new *available quantity*, or Q, for passing through another piece of wire afterwards included in the circuit*. Hence for a wire of constant S and of $n \times$ the length of another, we have (from formula A) the new

$$q = \frac{e^n \, . \, Q}{e^n + n \, . \, e^{n-1} \, . \, Q}, \text{ or, the new } q = \frac{e \, . \, Q}{e + n \, . \, Q} \quad \ldots \quad \text{(B}$$

When a copper wire having S = $\frac{1}{100}$ square inch, and L = 58 feet, completes the circuit of a battery in such action, that Q is equal to the electricity circulated by the combination of 0·1 grain of pure zinc per second, then q was found to be $\frac{1}{2}$ Q, or the electricity arising from the combination of 0·05 grain of zinc per second.

Now by applying this numerical datum, and the fact that increasing S and Q together increases q in the same ratio, by applying this to the above formula B, and reckoning L in feet, S in square inches or fractional parts of a square inch, and representing Q and q by grains of zinc used per second, we may deduce the following formulæ :—

$$L = 580 \, . \, S \, . \, \frac{Q - q}{Q \, . \, q},$$

* This reasoning will equally apply although the electric influence be propagated by vibrations in any medium, or even by any simultaneous affection or change of state in the particles of the conducting substance, but the convenient analogy of a current renders the subject much clearer.

$$S = \frac{L}{580} \cdot \frac{Q \cdot q}{Q - q},$$

$$Q = 580 \cdot S \cdot \frac{q}{580 \cdot S - L \cdot q},$$

$$q = 580 \cdot S \cdot \frac{Q}{580 \cdot S + L \cdot Q} \,*$$

which appears to accord with some results arrived at by Ohm, which I have subsequently seen; though he has not deduced formulæ of quite the same nature.

These formulæ are useful to solve many practical questions, such for instance as relating to the size of conducting wires, and power of batteries necessary to transmit a given quantity of electricity through a given distance, as in telegraphs; and in determining the ratio of the section to the length of helix wires in forming electro-magnets of various dimensions and proportions, so as to obtain a maximum of effect, which depends chiefly on the number of coils × the quantity of electricity in circulation.

The constant 580 in the formulæ applies only to copper wire unannealed; to adapt the formulæ to different metals we must vary the constant in the direct ratio of their conducting powers. The above calculations and experiments refer only to electricity of the intensity of a single cell of Daniell's constant battery. It is probable (judging from an experiment with electricity of *two* cells' intensity) that an increase in the intensity has the same effect on the transmission of the electricity, as a proportionate increase in the conducting power of the wire, as Ohm had previously deduced.

The ordinary form of galvanometer, by the deflection of the needle in a helix placed so as to be in the magnetic meridian, cannot determine the comparative amounts of two currents of electricity by any calculation from the degrees of deflection, unless previous data be obtained by other experiments, as the amount of deflective force exerted by any given current at different arcs of deflection (when balanced by ter-

* These formulæ were tested by the following experiments:—A length of copper wire (220 feet) was led about a room, double, that is, the returning half running close and parallel to the other part of the wire without touching: the doubled wire dipped into pairs of mercury cups at certain intervals; any of these could be connected or disconnected in an instant, by inserting or removing a bit of connecting wire, and thus the current was transmitted through various lengths of wire successively. The power of the battery was also varied by increasing the number of united cells, or diminishing the strength of the acid. A magnet galvanometer, included in the circuit, showed the quantity of electricity passing, either through the wire or without it.

restrial magnetism) is not as the tangents of these arcs, as would be the case if the deflective action of the helix on the poles of the needle were constant, for all positions of the needle, in amount as well as direction; but depends on the shape of the helix and the proportional length of the magnet placed within; because the action depends on the law, that *every portion of an electric current constantly tends to deflect a magnetic pole, in a direction at a right angle to the current itself, and also at a right angle to the straight line from that pole to the current, and with a force inversely as the square of the distance of the pole from that portion of the current.*

In the case of a square helix and a magnet nine-tenths of the average exterior and interior length of a side of the helix, the tangential force arising from the deflection by the helix varies with the angle of deflection, as here stated.

Degrees of the arc	0°	5°	10°	20°	30°	45°	70°	90°
Tangential deflective force	1·	0·74	0·56	0·36	0·236	0·112	0·02	0·00

I found a useful and accurate form of galvanometer for measuring and comparing the power of galvanic batteries, or currents which are not very feeble, to be, a helix and a thin flat magnet, so placed on an axis as to be deflected vertically when a current is transmitted, and weights applied in any way so as to retain the magnet in a horizontal position: the weights should act not far from the axis, so as to make the deflecting force more sensible by its greater leverage. The helix wire should be thick and as short as possible; three coils are enough, so as not to impede the current to be measured.

One cell on Daniell's constant principle, one foot high, and two inches and a half broad, with a zinc rod inserted nine inches in the liquid, and charged with a strong solution of sulphate of copper, and 7 × diluted ordinary sulphuric acid, at about 65° of Fahrenheit, and with the circuit quite completed, evolves a quantity of electricity due to the consumption of 0·04 grain of zinc per second. For cells of such a size, cylinders of wood prepared as in Mullins's battery can easily be turned less than one-tenth of an inch thick and ten inches long; they do not appear to impede the current much more than the gut used by Daniell, and are more durable and easy to manage.

When the cells are near 32° temperature the sulphate of zinc as it is formed consolidates on the zinc rod; although the zinc be well amalgamated and quite pure, and the solution quite fresh, the electric current acts by starts instead of being perfectly uniform, as it is at 60° or even at 50°.

Experiments on the Shape of Magnets.

Eight steel bars were made of nearly the same hardness and quality, being six inches long, half an inch broad, and of various thicknesses, from 0·5 inch to 0·02; they were all magnetized by the single touch, being repeated until they would become no stronger; the power was measured by the force exerted at the extremity to turn into the magnetic meridian, when placed at right angles to it; this force, which we will call fn, was ascertained by calculation from the formula

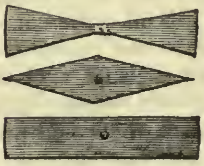
$$fn = W \cdot \frac{N^2 L}{824{\cdot}000}\left\{\begin{array}{l} W \text{ being the magnet's weight.} \\ N \text{ its number of single vibrations per minute.} \\ L \text{ the total length of the magnet in inches.} \end{array}\right.$$

The fn of all these magnets was the *same*, within the errors of experiment. Similar experiments were made with steel bars of 18, 6 and 2 inches in length, all being 0·5 inch broad; the fn of each was in the *exact ratio of their length*, as was partially ascertained by Captain Kater* and Coulomb.

Similar experiments were made with bars of 3, 0·5 and 0·25 inches in breadth, all being six inches long; the ratio of their fn was nearest that of the *square root of the breadth* (the smallest being too strong and the largest too weak; probably the ratio of the square roots would have been correct had each been rubbed with a magnet of corresponding breadth).

Similar experiments were made with bars of 0·5, 0·25 and 0·15 inch square, all being six inches long; the ratio of their fn was between that of the square and cube root of the breadth of their sides, being nearest the square root.

The relative fn of magnets of the accompanying forms, is as follows: their length was four inches, and breadth 0·8.



- - - 4
- - - 7
- - - 12

They were magnetized in the direction of the shade lines.

Effect of approximating Magnets.

Two bar magnets, six inches long, 0·5 inch broad, and 0·05 inch thick, were placed parallel with similar poles adjacent flatwise, one over the other at various distances, and their magnetic power ascertained by vibrations, according to the

[* Capt. Kater's paper on this subject will be found in Phil. Mag., vol. lix. p. 359.—EDIT.]

preceding formula; their power diminished as they approached, in the ratio here shown :—

Distance of magnets apart. inches.	Magnetic power. *f n.*
Infinite	1·
3·	0·970
1·	0·907
0·8	0·885
0·5	0·844
0·4	0·825
0·3	0·798
0·2	0·765
0·15	0·74
0·1	0·725
0·05	0·7.

But the loss was only temporary, as they recovered their former power immediately that they were separated. [Their distances apart are reckoned from the middle of the thickness of each; so that when they are said to be at a distance 0·05, they are in actual contact.]

A number of hardened steel sheet magnets, 8 inches long, 0·8 wide, and about 0·03 thick, were placed in a light framework of wood, one over another at equal distances, as in the preceding experiment, and the whole mass slung by a thread of parallel silk fibres, and the magnetic power ascertained by vibrations; this pile of magnets was kept one foot high by using fewer magnets when they were placed further apart*. The power of each individual magnet was diminished as they approached, in the ratio here shown :—

Distance of magnets apart. inch.	Magnetic power. *f n.*
Infinite	1·
1·	0·95
0·8	0·93
0·5	0·83
0·4	0·78
0·3	0·7
0·2	0·6
0·15	0·52
0·1	0·42
0·05	0·28 †.

* Only forty-eight magnets were used; therefore, when they were placed very close, the pile could not be made one foot high: but the effect of increasing the height of the pile, the distance between the magnets remaining the same, was ascertained; and so the effect of a pile one foot high was calculated from experiments with one of a less height.

† The results given in the second column, are always comparative with the aggregate power of the same magnets when placed far apart.

But it is very trying to have two-thirds of the magnetic power *temporarily* suppressed by mutual proximity, and they would probably soon lose permanently a portion of it by any slight concussion.

The experiments were gone over three times, and the mean results taken. The magnets were placed at their closest distance before beginning, so that further permanent loss of magnetism might not take place during the experiments. I believe the third place of decimals in the first table cannot be trusted.

From the foregoing table and observation following, it appears that power is not gained by too great an approximation of plates of a compound magnet, and the thinner the plates the better.

W. Petrie.

LV. *On a Theorem given in the Philosophical Magazine for August.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE "new theorem" which is applied in the August Number of your Magazine to determine the development of $\cos n\,\theta$ in descending powers of $\cos \theta$, may be at once deduced from one of the fundamental formulæ in finite differences, as follows:—the formula referred to is

$$\Delta^n u_x = u_{x+n\,h} - n\,u_{x+\overline{n-1}\,h} + \frac{n\,\overline{n-1}}{1\,.\,2}\,u_{x+\overline{n-2}\,h} - \&c.$$

$$\text{or } (-1)^n\ \Delta^n u_x = u_x - n\,u_{x+h} + \frac{n\,\overline{n-1}}{1\,.\,2}\,u_{x+2h} - \&c.,$$

where u_x is any function of x, and $h = \Delta x$.

Now let u_x be of m dimensions in x, and $= (a-x)\,(b-x)$ $(c-x)\dots(l-x)$, then it is evident, that if $n > m$, $\Delta^n u_x = 0$

$$\therefore\ 0 = u_x - n\,u_{x+h} + \frac{n\,\overline{n-1}}{1\,.\,2}\,.\,u_{x+2h} - \&c.,$$

In this equation put $x = 0$,

$$\therefore\ 0 = u_0 - n\,u_h + \frac{n\,\overline{n-1}}{1\,.\,2}\,.\,u_{2h} - \&c.,$$

which is the theorem to be proved.

With respect to the use of this theorem in expanding $\cos n\,\theta$

according to descending powers of cos θ (where n is a positive integer), it seems extremely doubtful whether anything would be gained by its introduction in an elementary work; the method given in the ordinary treatises on trigonometry depending on the logarithmic series, is on every account preferable.

In the same article in your Number for August there is given a proof that $\Delta^n x^m = 0$ when $m < n$.

If the author of it will only turn to art. 885 in Lacroix, I am convinced that he will regret having offered his new demonstration. Believe me, yours sincerely,

Trinity College, Cambridge, J. E.
Aug. 24, 1841.

LVI. *On a Mode of deducing the Equation of* Fresnel's *Wave.*

By Sir WILLIAM ROWAN HAMILTON, *LL.D.*, *P.R.I.A.*, *Member of several Scientific Societies at Home and Abroad, Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland**.

THE following does not pretend to be the *best*, but merely to be *one* way, of deducing the known equation of Fresnel's wave, from a known geometrical construction. It requires only the first principles of the application of algebra to the geometry of three dimensions, and does not introduce any of the geometrical properties of the auxiliary ellipsoid employed, except those which are immediately suggested by the equation of that ellipsoid. It has, therefore, in the algebraical point of view, a certain degree of directness, although it might be rendered easier and shorter by borrowing more largely from geometry.

1. The known construction referred to is thus enunciated by Sir John Herschel, in his Treatise on Light, *Encyclopædia Metropolitana*, article 1017. "M. Fresnel gives the following simple construction for the curve surface bounding the wave in the case of unequal axes, which establishes an immediate relation between the length and direction of its radii. Conceive an ellipsoid having the same semiaxes a, b, c; and having cut it by any diametral plane, draw perpendicular to this plane from the centre two lines, one equal to the greatest, and the other to the least radius vector of the section. The loci of the extremities of these perpendiculars will be the surfaces of the ordinary and extraordinary waves."

2. The coordinates of the wave being $x\ y\ z$, and those of the ellipsoid X Y Z, we have the six equations,

* Communicated by the Author.

$$\frac{X^2}{a^2} + \frac{Y^2}{b^2} + \frac{Z^2}{c^2} = 1, \quad . \quad . \quad . \quad . \quad (1.)$$

$$\frac{X\,dX}{a^2} + \frac{Y\,dY}{b^2} + \frac{Z\,dZ}{c^2} = 0, \quad . \quad . \quad (2.)$$

$$x\,X + y\,Y + z\,Z = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$x\,dX + y\,dY + z\,dZ = 0, \quad . \quad . \quad . \quad (4.)$$

$$X\,dX + Y\,dY + Z\,dZ = 0, \quad . \quad . \quad (5.)$$

$$x^2 + y^2 + z^2 = X^2 + Y^2 + Z^2; \quad . \quad . \quad . \quad . \quad (6.)$$

between which we are to eliminate X, Y, Z, and the ratios of their differentials.

3. The equations (1.) and (2.) are satisfied by assuming

$$X = a \sin\theta \cos\phi,\ \ Y = b \sin\theta \sin\phi,\ \ Z = c \cos\theta; \quad (7.)$$

and then the equation (3.) gives

$$\tan\theta = \frac{-c\,z}{a\,x \cos\phi + b\,y \sin\phi}; \quad . \quad . \quad . \quad . \quad (8.)$$

while the comparison of the two values of $\tan\theta \frac{d\phi}{d\theta}$, deduced from (4.) and (5.), gives

$$\frac{a\,x \cos\phi + b\,y \sin\phi - c\,z \tan\theta}{a\,x \sin\phi - b\,y \cos\phi} = \frac{a^2 \cos\phi^2 + b^2 \sin\phi^2 - c^2}{(a^2 - b^2) \sin\phi \cos\phi}; \quad (9.)$$

and the equation (6.) becomes

$$(x^2 + y^2 + z^2)(1 + \tan\theta^2) = (a^2 \cos\phi^2 + b^2 \sin\phi^2) \tan\theta^2 + c^2. \ (10.)$$

It remains therefore to eliminate θ and ϕ between the three equations (8.) (9.) (10.).

4. Substituting for $\tan\theta$, in (9.) and (10.), its value given by (8.), we easily obtain

$$A \tan\phi + B \operatorname{cotan}\phi = C; \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

$$A' \tan\phi + B' \operatorname{cotan}\phi = C'; \quad . \quad . \quad . \quad . \quad . \quad (II.)$$

if we put for abridgement

$$A = (c^2 - b^2)\, a\,b\,x\,y;$$

$$B = (a^2 - c^2)\, a\,b\,x\,y;$$

$$C = (b^2 - c^2)\, a^2 x^2 + (c^2 - a^2)\, b^2 y^2 + (b^2 - a^2)\, c^2 z^2;$$

$$r^2 = x^2 + y^2 + z^2;$$

$$A' = r^2 (b^2 y^2 + c^2 z^2) - c^2 b^2 (y^2 + z^2);$$

$$B' = r^2 (a^2 x^2 + c^2 z^2) - c^2 a^2 (x^2 + z^2);$$

$$C' = -2 (r^2 - c^2)\, a\,b\,x\,y.$$

And eliminating ϕ between the equations (I.) and (II.), we find:

$$(A\,B' - A'\,B)^2 + (A\,C' - A'\,C)(B\,C' - B'\,C) = 0; \quad (III.)$$

a form for the equation of the wave, which we have now only to develope and depress.

5. Expanding it first under the form

$$W_8 + W_{10} + W_{12} = 0,$$

in which W_8, W_{10}, W_{12} are, respectively, homogeneous functions of x, y, z, of the 8th, 10th, and 12th dimensions, we soon discover that these three functions have a common factor, of the 8th dimension, namely, $c^2 z^2 r^2 R^2$, in which

$$R^2 = C^2 + 4 a^2 b^2 (c^2 - a^2)(c^2 - b^2) x^2 y^2,$$

C having the same meaning as in (I.), so that

$$R^2 > 0, \text{ if } c^2 > b^2 > a^2, \text{ or if } c^2 < b^2 < a^2,$$

conditions which we may suppose to be satisfied. And rejecting, as evidently foreign to the question, this common factor $c^2 z^2 r^2 R^2$, the known equation of the wave results, under the form

$$u_0 + u_2 + u_4 = 0, \quad \ldots\ldots\ldots \quad \text{(IV.)}$$

in which $u_0 = a^2 b^2 c^2$,

$$u_1 = -\{a^2(b^2+c^2)x^2 + b^2(c^2+a^2)y^2 + c^2(a^2+b^2)z^2\},$$

$$u_2 = (x^2+y^2+z^2)(a^2x^2+b^2y^2+c^2z^2).$$

6. The foregoing investigation is taken from a manuscript Report which I had the honour of drawing up in July 1830, when, in conjunction with the late and present Provosts of Trinity College, Dublin, I was appointed to examine the first communication of Professor MacCullagh to the Royal Irish Academy, since published in the second part of the sixteenth volume of the Transactions of that body. A far more concise and elegant deduction of the same known equation of the wave from the same geometrical construction, depending, however, a little more on the geometrical properties of the ellipsoid, has since been communicated by Professor MacCullagh himself, and is published in the second part of the seventeenth volume of the Transactions of the same Academy. Others have published other demonstrations.

My own mode of deducing the equation of the wave from the principles of Fresnel, without any reference to the ellipsoid above referred to, may be seen in the 'Third Supplement' to my Theory of Systems of Rays contained in the first part of the last-mentioned volume.

Observatory of Trinity College, Dublin,
October 13, 1841.

P.S. Since writing out and sending off the foregoing paper, I have had opportunity to refer to Fresnel's own deduction of the same equation of his wave from the same geometrical construction, entitled "Calcul très simple qui conduit de l'équation d'un ellipsoide à celle de la surface des ondes." (*Mém. de l'Acad. des Sci. de l'Inst. Royale de France*, tom. vii., page 137.) It is much simpler than mine, and nearly coincident with that of Professor MacCullagh, but seems to have been overlooked by both of us.

LVII. *Remarks on* M. Mossotti's *Theory of Molecular Action**.

SOME years since a paper by M. Mossotti, on the forces which regulate the internal constitution of bodies, attracted a good deal of attention among scientific men. A translation of it appeared in the third part of the 'Scientific Memoirs.' The author's object was to develope the views of Franklin and Æpinus with respect to electricity. It is well known, that in the form which the latter of these philosophers gave to the single fluid hypothesis, a mutual repulsion is supposed to exist among the material molecules of which bodies are composed, as well as among the particles of the electrical æther; while these particles are supposed to be attracted by the material molecules. Assuming that all the forces required by the hypothesis vary directly as the mass and inversely as the square of the distance, is it possible to establish stable equilibrium among the molecules of which bodies are composed? This is the question which M. Mossotti proposed to determine.

A priori, stable equilibrium certainly appears to be possible; for, as the author remarks, if the molecules of matter surrounded by their atmospheres attract each other when at a distance, and exercise a repulsive force when brought nearer together, there must be some intermediate position of stable equilibrium. But although M. Mossotti's general view may be correct, I believe it will be found that his analysis is erroneous. As the subject of molecular attraction possesses at the present day a good deal of interest, I trust that I may be permitted to develope at some little length my objections to M. Mossotti's investigation. For this purpose it will be necessary to follow his analysis as far as the reduction of the problem to equations.

Let f be the coefficient of the repulsion between any two atoms of the æther; g the coefficient of the attraction between an atom of the æther and a material molecule; and γ the coefficient of the repulsion between two material molecules.

Again, let q be the density of the æther at a point $x\ y\ z$, and ε the elastic force or pressure at the same point, referred to the superficial unit. Let π be the density at a point $\xi\ \eta\ \zeta$ of a molecule supposed to be of certain though very small dimensions.

All the forces of the system may be represented by the partial differential coefficients of the following definite integrals:—

* Communicated by the Author.

$$\mathrm{F} = \iiint \frac{f q' \, dx' \, dy' \, dz'}{\{(x'-x)^2 + (y'-y)^2 + (z'-z)^2\}^{\frac{1}{2}}},$$

$$\mathrm{G} = \iiint \frac{g \pi \, d\xi \, d\eta \, d\zeta}{\{(\xi - x)^2 + (\eta - y)^2 + (\zeta - z)^2\}^{\frac{1}{2}}},$$

$$\Phi = \iiint \frac{g q' \, dx' \, dy' \, dz'}{\{(x' - \xi)^2 + (y' - \eta)^2 + (z' - \zeta)^2\}^{\frac{1}{2}}},$$

$$\Gamma_\gamma = \iiint \frac{\gamma \pi_\gamma \, d\xi_\gamma \, d\eta_\gamma \, d\zeta_\gamma}{\{(\xi_\gamma - \xi)^2 + (\eta_\gamma - \eta)^2 + (\zeta_\gamma - \zeta)^2\}^{\frac{1}{2}}}$$

(γ subscript refers to the 5th molecule).

These integrals being taken between suitable limits, we have for the equilibrium of the æther the equations

$$\left.\begin{aligned} \frac{d\varepsilon}{dx} &= -q\frac{d\mathrm{F}}{dx} + q\Sigma\frac{d\mathrm{G}}{dx} \\ \frac{d\varepsilon}{dy} &= -q\frac{d\mathrm{F}}{dy} + q\Sigma\frac{d\mathrm{G}}{dy} \\ \frac{d\varepsilon}{dz} &= -q\frac{d\mathrm{F}}{dz} + q\Sigma\frac{d\mathrm{G}}{dz} \end{aligned}\right\}; \quad \ldots\ldots\ldots\ldots \quad (1.)$$

and for that of the centre of gravity of a molecule,

$$\left.\begin{aligned} \iint \varepsilon \, d\eta \, d\zeta &= \iiint \pi \frac{d\Phi}{d\xi} d\xi \, d\eta \, d\zeta \\ &\quad - \Sigma \iiint \pi \frac{d\Gamma_\gamma}{d\xi} d\xi \, d\eta \, d\zeta \\ \iint \varepsilon \, d\xi \, d\zeta &= \iiint \pi \frac{d\Phi}{d\eta} d\xi \, d\eta \, d\zeta \\ &\quad - \Sigma \iiint \pi \frac{d\Gamma_\gamma}{d\eta} d\xi \, d\eta \, d\zeta \\ \iint \varepsilon \, d\xi \, d\eta &= \iiint \pi \frac{d\Phi}{d\zeta} d\xi \, d\eta \, d\zeta \\ &\quad - \Sigma \iiint \pi \frac{d\Gamma_\gamma}{d\zeta} d\xi \, d\eta \, d\zeta \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (2.)$$

In both these systems of equations Σ refers to the various molecules whose equilibrium we are considering; in (1.) it extends to all the molecules; in (2.) to all but one.

The double integrals in (2.) are to be extended to the whole surface of the molecule under consideration, and the triple integrals to its whole volume. Such are M. Mossotti's equations. Let us begin by examining the meaning of the several terms of those of the second system. The last terms of these

three equations represent the total actions (respectively parallel to the axes of co-ordinates) exerted by all the molecules of the system but one, upon that one. Consequently the other six terms must refer to the action of the æther upon the molecule under consideration. Accordingly the second terms express the attraction (according to the assumed law of the inverse square of the distance) exerted by the whole body of æther upon the molecule in question. But this, according to the hypothesis, comprises the whole action of the æther upon the molecule. What then do the first terms represent? They represent the components parallel to the axes of the total pressure exerted by the æther over the surface of the molecule. Hence it appears that the idea of fluid pressure enters into the formation of our equations. But, on the molecular hypothesis of the constitution of bodies, what is fluid pressure but a form of molecular action? Hence the first terms of our equations, as well as the second, refer to molecular action between the æther and the material particles. We are thus reduced to the following alternative:—either the molecular action expressed in the first terms is included in that expressed in the second, in which case the first terms are superfluous, and the equations are incorrect; or, beside the forces given in the hypothesis, we must take into account certain other forces, whose nature is wholly unknown, namely, those by which the pressure on the molecules is produced. As the object of the hypothesis from which we set out is to explain the nature of molecular action, it is clear that we are not at liberty to introduce the agency of unknown molecular forces. Thus the only conclusion we can adopt is that M. Mossotti's second system of equations is not in accordance with his hypothesis, and that the latter does not admit of being modified so as to produce an agreement. Let us now examine the first system of equations, those, namely, which refer to the equilibrium of the æther. They are, in reality, only the ordinary equations of fluid equilibrium, which are generally put in the following form:—

$$\frac{dp}{dx} = \rho\, X \quad \frac{dp}{dy} = \rho\, Y \quad \frac{dp}{dz} = \rho\, Z \ldots\ldots\ldots\ldots\ldots\ldots (3.)$$

where p is what M. Mossotti denotes by ε; ρ is equivalent to his q, and X, Y, Z, are the forces impressed upon the fluid. If we consider with some degree of attention the signification of these equations, we shall find that they express the conditions of the equilibrium of an element of the fluid, taken as a continuous mass. The circumstances under which we are justified in considering a congeries of molecules as a continuous

mass, are well known, and need not here be discussed. To return, the equilibrium of the element $d\,x\;d\,y\;d\,z$ of the fluid is maintained by the respective equality of the impressed moving forces $X\rho\;d\,x\;d\,y\;d\,z$, $Y\;\rho\;d\,x\;d\,y\;d\,z$, $Z\;\rho\;d\,x\;d\,y\;d\,z$, to the differences of pressure over the opposite faces of the element, or to $\frac{d\,p}{d\,x}\,d\,x\,.d\,y\,d\,z$; $\frac{d\,p}{d\,y}\,d\,y\,.\,d\,z\,d\,x$; and $\frac{d\,p}{d\,z}\,d\,z\,.\,d\,x\,d\,y$. Now this pressure is nothing but the result of molecular action between contiguous particles. Hence it is clear that the expressions X. Y. Z do not include all the forces which act upon the particles of the fluid: they exclude precisely the molecular forces.

But if we examine the terms on the right-hand side of (1.), we shall find that they include *all* the forces, of whatever kind, which act upon the particles of the æther. The conclusion from this is obvious, and coincides with that which we derived from the consideration of (2.).

In fact the errors in both cases arise from the same source, namely, that after considering explicitly all the forces of the system, the author introduces the idea of fluid pressure, itself the result of a particular mode of molecular action.

If this were a question to be decided by a reference to authority, we might remark, that in the various memoirs which have of late years been published on the molecular view of the undulatory theory of light, the luminiferous æther is considered simply as a congeries of molecules; and further, that where, as in a memoir by Poisson, in those of the Institute for 1830, and in some of Cauchy's, the pressure of fluids or the tension of solids is introduced in connexion with molecular action, they appear as results derived from it, and not as co-ordinate principles.

The preceding observations contain little that is not familiar to most of those who take any interest in mathematical physics, but perhaps the celebrity of M. Mossotti's memoir may give them some degree of interest, and at any rate they may serve to point out the necessity of distinct views of the connexion between the theory of molecular action and the ordinary principles of equilibrium.

R. L. E.

September 24, 1841.

LVIII. *Some Observations on* Dr. Brett *and* Mr. Smith's *Experiments on the alleged Conversion of Carbon into Silicon**. *By* Dr. Samuel M. Brown.

To Richard Phillips, Esq.

Sir,

As Dr. Brett and Mr. Smith have made the Philosophical Magazine the medium of publication for their "Experiments on the alleged Conversion of Carbon into Silicon," I request you to do me the honour of giving the same publicity to these few and brief observations on their letter, while I gladly acknowledge the liberal style of investigation in which the gentlemen in question have undertaken and executed their task.

Your Correspondents specify, on the very threshhold, that the "paracyanogen employed by them was produced by the decomposition of hydrocyanic acid;" and this is sufficient to render almost all their experiments of no value. I do not, indeed, understand the statement. If it be meant that their paracyanogen was prepared by the spontaneous decomposition of anhydrous hydrocyanic acid, they must prove that paracyanogen can be so produced at all; for M. Gay-Lussac found that hydrocyanic acid is wholly resolved into ammonia and a brownish-black matter, inferred to be "un azoture de carbone," which cannot be paracyanogen if ammonia be the only other product. If, again, their paracyanogen were a product of the decomposition of aqueous hydrocyanic acid, I am ready to show that it was a low hydrate of paracyanogen, which I never made any experiments upon; but in such a case the burden of proof lies on them. Moreover, howsoever the subject of their experiments was prepared, (and in a matter affecting so vital a truth or untruth as this is they ought surely to have been more specific,) if it were not prepared exactly in the same way as my own, their results can have no other relation to mine than that of parallel lines, incapable of meeting and merging indeed, but by no means diverging and contradictory. Accordingly, nothing can be said of their experiments till, at the middle of p. 299, they determine to avoid every objection by employing paracyanogen prepared from bicyanide of mercury, and make one with what seems to be the substance I used; one experiment.

Here I find no sufficient evidence in the single sentence, in which the authors describe the preliminary process and its product, that the former was rightly performed, or that

* Philosophical Magazine, Oct. 1841, p. 295.

the latter was true paracyanogen; and I do know that this process is so difficult of performance, that it was not till after long practice that my assistants, Messrs. Craig and Thomas Brown, acquired facility in working it. Solubility in sulphuric acid is no test; blackened half-charred cyanide of mercury, paracyanide of mercury, and most ex-organic carbonaceous substances, are all dissolved by that menstruum as well as paracyanogen. It appears that "the residue was black and coherent;" it should have been extremely incoherent. I do not think Mr. Smith employed the same material as I used in this experiment, which is the only attempt at a literal repetition of any of mine in the whole list on free paracyanogen. The paracyanogen I worked with was all prepared in the hammer-iron paracyanogen-tube, which is described in my paper on the production of silicon. Will neither Dr. Brett nor Mr. Smith get one made?

It appears that the next set of experiments was "On the formation of compounds of silicon with copper, iron and platinum by the reaction of these metals on paracyanogen" (p. 300); but they were made with paracyanogen to which I have already excepted, and that is enough. In addition to that quite fatal objection, the first is equal to nothing according to Dr. Brett himself; the second and third, by Mr. Smith, are not performed according to my directions; and in all the three the investigation is far from being rigid. May I be allowed to state my respectful conviction, that the authors have not striven as they might? My experiments cost me eighteen months' incessant labour and many a failure. Even so late as last November and December I tried to effect the alleged transformation before a celebrated physician and chemist six weeks in vain; but at last succeeded, or, as I must say now, appeared to succeed.

For the next experiments (p. 301), on the ferrocyanide of potassium, let such as are interested in the subject judge whether they be sufficient repetitions of mine. I may mention, in passing, that the two apparatus described in this part of your Correspondents' letter, viz. an iron tube closed with an iron plug, and a gun-barrel protected by luting and well secured at the orifice, were the very instruments that foiled me last November and December. There must be free enough exit for nitrogen; I used large crucibles of hammer-iron with loose lids and porous lutes. I do not allude to the first experiment of this set (p. 301), for it is original on the part of Mr. Smith. This brings us down to p. 304.

The last experiment but two (p. 304) just confirms Mr. Johnstone's observation, already confirmed by myself, that

ignigenous paracyanogen gives off cyanogen when heated! The last but one is wholly exceptionable on account of the material; and consequently, the last paragraph but one (p. 305) is not to the point. The last of all is a feeble trial, made on a wrong subject, and attended by no significant result.

In conclusion, this is all I can oppose to the authority of Dr. Brett and Mr. Smith. I trust it is enough to preserve the right-minded scepticism of chemists on the subject at issue, which is either so momentous as to be worth a world of cautious inquiry, or so unimportant as not to merit even condemnation. Meanwhile, I beg your leave to take this opportunity of offering a few suggestions on the right repetition of my experiments, which I confess to be necessary on account of the extreme condensation of my memoirs; a quality, which must be attributed to my inexperience on one hand, and a settled aversion to many words on the other.

If the apparent transformation be real, it is manifest that the rationale must be very subtle to find, however simple it may yet be found to be, and that it is very likely that the success of the processes may depend on contingencies which are the least obvious. I often failed; at length far oftener succeeded; and have described the operations in successful cases, trusting that I have included the essential circumstances of success, but knowing very well that I may not have done so. Hence the necessity of copying the very letter of the text; and, hence, too, my humble right of suggesting the propriety of not concluding too hastily against my special positive results. Chemists, who are willing to work, will not easily be deluded by silicated vessels, dirty crucibles, or foul potassa. In chemical investigation it is very difficult to discover new truth; but for every one who feelingly knows that it is so, it is also difficult to be very much misled.

As it is evident that, so far as I was led to perform these experiments by hypothetical insight, true or imaginary, the likelihood of success depended on the correctness of the view of the constitution of paracyanogen exhibited in the paper on that substance, scrupulous care must be taken not to infringe on that constitution by any experimental accident, such as ever so slight access of air during the prolonged heatings; inasmuch as for every equivalent of carbon that may be surreptitiously drawn off, say in the form of carbonic oxide, three equivalents are lost for conversion, and one less of silicon can be produced. I have wasted a great deal of paracyanogen in this way: and it was only last week that I hit upon a lute capable of hindering the paracyanide of a certain metal from

being wholly changed into carbonic oxide, nitrogen, and metallic oxide during a protracted subjection to the red heat of iron in a porcelain crucible, after having made a great many trials in vain.

Paracyanogen is like carbon, boron, and silicon, in presenting two distinct forms, the unignited and the ignited; with all the intermediate conditions. In the former state it gives nitrogen away at the incipient red heat of iron, though slowly and without force enough to overcome any considerable pressure; while in the latter it requires at least the white heat. On this account ignited paracyanogen may be mistaken for ignited carbon, and an operation said to have failed, which would have succeeded if it had been finished. Paracyanogen is in every way a tantalizing thing to make experiments upon.

My white heat for copper is the temperature at which the metal is white to my eye, and yet solid; when it is swelled out to the utmost verge of solidity, and is about to fall down liquid. This, too, needs management. As to the fusion of sulphate of lime, it is needless to speak of every contingency. To know the accidental from the essential is the problem of scientific authorship.

In fine, the alleged conversion of carbon into silicon is before the world, at least unencumbered by presumptuous explanations, and not disgraced by anything like a challenge. If others fail to procure the same results, I shall be sorry; but so long as my observations continue to lead me on to others of a more and more practical kind, I shall be very well content to have my first publication regarded as a plenary mistake, and will refrain from writing any more till it shall have become impossible not to succeed in the performance of my operations. I have, Sir, the honour to be,

Your obedient Servant,

SAMUEL MORISON BROWN.

3 Dartmouth Row, Blackheath, near London.

LIX. *Description of an Electric Thermometer. By* E. SOLLY, *Jun., Esq.*

To Richard Taylor, Esq.

MY DEAR SIR,

I AM induced to send you the following account of a little thermo-electric arrangement, believing that it may be interesting to some of your readers; for although there is little or no novelty in the principles on which its action depends, I am not aware that it has been before practically employed.

I had for some time experienced considerable inconvenience

in conducting certain experiments requiring a long-continued and uniform degree of heat, from the difficulty of regulating the temperature of my furnace, and the constant uncertainty whether everything was proceeding satisfactorily during my absence from the laboratory. I had in consequence often thought of the possibility of so arranging a little thermo-electric apparatus that it might serve as an index of the rate of combustion and consequent heat of the furnace, by the deflection of a galvanometer at a distance from the source of heat. A small thermo-electric battery might be so placed that the one series of joints or solderings should be constantly exposed to the heated surface of the furnace; but a serious obstacle presented itself to any contrivance of this kind, which was the difficulty of keeping the alternate joints of the battery cool: a current of electricity would doubtless be evolved in consequence of the difference of temperature existing between the two sides of the battery, but of course as the heat would gradually traverse from the hotter to the cooler side, it would greatly diminish and modify the results, and thus present false indications of temperature; whilst even if it were possible to keep the one side of the battery cool, either by water or by any other means, yet the value of the deflection of the galvanometer would be always uncertain, as the difference between the two sides of the battery could never be ascertained unless the exact reduction of temperature thus caused were correctly known.

After one or two unsuccessful attempts to overcome this objection, I laid aside the battery and substituted in its place a single pair of metallic elements, which I found gave abundance of power, and was not liable to the defect which the use of the battery involved.

A piece of copper wire, one twenty-fourth of an inch in diameter, and of sufficient length to reach from the furnace to my ordinary sitting-room, was joined by twisting the ends to a similar wire of soft iron, the ends of both having been previously well cleaned with sand-paper. The two wires were then secured in a convenient manner by small nails to the walls of the rooms they had to pass through, care being taken that they were not anywhere in contact with each other, except at the two extreme points of junction; the one of these was so placed in the flue of the furnace that it was completely exposed to the action of the hot air and smoke at that part where the flue left the body of the furnace, whilst the other joint was in my room in contact with a thermometer, and surrounded with cotton, so as to render it as little as possible liable to sudden changes of temperature. The copper wire

was then divided about a foot from the joint thus protected; the two ends of the wire were connected with the extremities of a galvanometer coil, and the apparatus was complete.

A metallic circuit was thus made consisting of two elements, the one being the iron-wire, and the other the copper-wire, including the additional length of copper wire in the coil of the galvanometer. The one joint or point of contact was of course always far hotter than the other, and would necessarily remain so, so long as the fuel in the furnace continued to burn, and would be dependent on the rate of combustion in the furnace; whilst the other junction would always remain very nearly at the temperature of the air, and its variations could be readily known by the thermometer in contact with it. A current of electricity was thus generated, proportioned to the difference of temperature between the two joints, and a deflection of the galvanometer was caused, which increased when the furnace became hotter, decreased when it cooled, and at all times indicated accurately the changes of temperature taking place, thus giving me a thermometer which indicates, without my moving from the table, the exact rate of combustion going on in the furnace, which is fifty yards distant from the indicator. I believe that it is commonly supposed that weak thermo-electric currents cannot be well made to traverse small wires of any length, and this is probably the reason why this beautifully manageable power has been so little employed for practical uses. I have received so much satisfaction from the arrangement just described, that I am convinced it would be found a very useful indicator of temperature in stoves, flues, and hot pipes, in many situations where a common thermometer is inapplicable.

The cost of such an apparatus must necessarily be more expensive than any thermometer, but then it must be remembered that it does far more than an ordinary thermometer, giving us the means of knowing the temperature of a stove or furnace at a distance, giving us indications of the least change or variation in the source of heat, with even greater certainty and distinctness than a thermometer; and besides, showing these changes so rapidly that we know whether it is becoming hotter or colder, before a thermometer placed on the outside of the stove indicates any change. I have observed, on comparing the thermo-electric with an ordinary thermometer placed on the iron plate forming the top of the furnace, that if the ash-pit door were closed, or the draught in any other way diminished, the deflection of the galvanometer was immediately reduced, whilst the external thermometer continued to rise for some little time; and

that the indications given by the galvanometer of the augmentation or decrease of temperature in the furnace, always preceded the same indications from the mercurial thermometer on the top.

I am, dear Sir, yours very truly,

London, Oct. 20, 1841. EDWARD SOLLY, Jun.

LX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 325.]

Feb. 24, 1841. A PAPER, entitled "Description of parts of the Skeleton and Teeth of five species of the genus Labyrinthodon, from the new red sandstone of Coton End and Cubbington Quarries; with remarks on the probable identity of the Cheirotherium with that genus of extinct Batrachians," by Richard Owen, Esq., F.G.S., F.R.S.

In a paper read on the 20th of January, Mr. Owen described the peculiarities in the structure of the teeth of the Labyrinthodon (see *ante*, p. 315); and having been favoured by Dr. Lloyd, since that paper was written, with the loan of all the reptilian remains obtained from the new red sandstone of Warwick and Leamington, deposited in the Museums of those towns, and having been liberally permitted by the Committees of the Institutions to examine the teeth by the microscopic test, he gives, in this paper, a minutely detailed description of the fragments submitted to his examination, and points out their relative connexion to each other, and the laws by which he has been enabled to determine that they all belong to the genus Labyrinthodon, and [are] confirmatory of the Batrachian nature of the Wurtemberg fossil.

The specimens which Mr. Owen has examined are referable to five species, to which he has applied the names,—1. *Labyrinthodon salamandroides*, 2. *L. leptognathus*, 3. *L. pachygnathus*, 4. *L. ventricosus*, and 5. *L. scutulatus*; and he describes successively the characters exhibited by the bones assignable to the 2nd, 3rd and 5th species.

Labyrinthodon leptognathus.—The remains which Mr. Owen considers as portions of this species, consist of fragments of the upper and lower jaws, two vertebræ, and a sternum. They were found in the sandstone quarries at Coton End, near Warwick.

The portions of the upper jaw show that the maxillary or facial division of the skull was broad, much depressed and flattened, resembling the skull of the gigantic Salamander and of the Alligator; and the outer surface of the bones was strongly sculptured, as in the Crocodilian family, but of a relatively larger and coarser pattern. The fragment described contains the anterior moiety of the single row of small teeth, or 30 sockets, and the base of one of the great anterior tusks. The bases of the serial teeth project directly from

the outer wall of the shallow socket, there being no alveolar ridge external to it. The large anterior fang is three times the size of the first of the serial teeth, and the size of these gradually diminish as they are placed further back; the length of the common-sized being about two lines, and the greatest breadth one-third of a line. The apical two-thirds of each tooth is smooth, but the basal third is fluted, and anchylosed to the outer wall of the socket. The breadth of the upper jaw, opposite the middle of the dental series, was two inches six lines; in proceeding backwards the jaw gradually expands to three inches, and in proceeding forwards narrows, but in a less degree towards the anterior extremity, and then slightly widens or inclines outwards on account of the large tusks. Where the upper jaw is entire, a portion next the median suture, four lines in breadth, is separated from the maxillary bone by a longitudinal harmonia, and corresponds with the position of the nasal bone in the Crocodile. On comparing the structure of the cranium of the Labyrinthodon with the Batrachian condition of the same part, Mr. Owen shows that an important difference will be found to exist. In both the caducibranchiate and perennibranchiate species, the upper maxillary bones do not extend horizontally over the upper surface of the skull, but leave a very wide interval between the maxillary and nasal bones; and the palatal processes of the former contribute as little to form the floor of the nasal cavity: in the Crocodiles, on the contrary, the palatal processes of the maxillary bones extend horizontally inwards, and meet at the middle line of the roof, forming an unbroken floor to the nasal cavity. In the Labyrinthodon the superior maxillary bones, as already shown, extend inwards to the nasal bone, constituting with it a continuous roof to the nasal cavities; but the palatal processes, instead of reaching to the middle line, as in the Crocodiles, are very narrow, as in the Batrachia. The osseous roof of the mouth is principally composed of a pair of broad and flat bones, analogous to the divided vomer in Batrachia, but of much greater relative extent, approaching, in this respect, those of the Menopome, and defending the mouth with a more extensive roof of bone than exists in any Lacertian reptile: "physiologically, therefore," observes Mr. Owen, "the Labyrinthodon, in this part of its structure, comes nearest to the Crocodile; but the structure itself, morphologically, is essentially Batrachian." In the Menopome and gigantic Salamander, a row of small teeth extends transversely across the anterior extremity of the vomerine bones: and the occurrence in the Labyrinthodon of a similar row, consisting in each palatine bone of three median small teeth and two outer larger ones, marks most strongly its Batrachian nature; and from the outermost tooth a longitudinal row of small and equal-sized teeth is continued backward along the exterior margin of the palatine bone. The whole of this series of palatal teeth is nearly concentric with the maxillary teeth.

In Lacertine reptiles the examples of a row of palatal teeth are rare, short, and situated towards the back of the palate, upon the pterygoid bones, as in the Iguana and Mosasaur. In Batrachia the

most common disposition of the palatal teeth is a transverse row placed at the anterior part of the divided vomer in Frogs, the Menopome and gigantic Salamander, and at the posterior part in certain toads. In the Amphiume*, on the contrary, the palatal teeth form a nearly longitudinal series along the outer margin of the palatine bones. The Labyrinthodon, as already shown, combines both these dispositions of the palatal teeth. The posterior palatine apertures are more completely circumscribed by bone than in most Batrachians, occupying the same relative position as in the Iguana. The posterior margin only of one of the anterior apertures is exhibited in this specimen, but from its curve Mr. Owen infers that the two apertures were not confluent, as in the Crocodile, the Frog, or the Menopome, but that they were distant, as in the Iguana.

From the physiological condition of the nasal cavity Mr. Owen is disposed to believe that the Labyrinthodon differed from the Batrachians and resembled the Saurians, in having distinct posterior nasal apertures surrounded by bone, and that its mode of respiration was the same as in the higher air-breathing reptiles. In the shedding and renewal of the maxillary and the transverse palatal teeth, Mr. Owen shows that the process took place alternately in each row, as in many fishes, whereby the dental series is always kept in an efficient state.

The author then describes a portion, sixteen inches long, of the left ramus of an under jaw from the Warwick sandstone, and considered to belong to the same species as the bone just described. It is slender and straight, and the symphysial extremity is abruptly bent inwards, and it presents, Mr. Owen says, almost as striking a Batrachian character as any of the bones just mentioned. The angular piece is of great breadth, extending on both sides of the jaw, and is continued forward to near the symphysis, forming the whole of the inferior part of the jaw, and extending upon the inner as far as upon the outer side of the ramus, the inner plate performing the function of the detached os operculare in the jaw of Saurians. The dentary bone is supported upon a deep and wide groove along the upper surface of the angular piece, which also projects beyond the groove, so as to form a strong convex ridge on the external side of the jaw, below the dentary piece. This character, which in the large bull-frog (*Rana pipiens*) is confined to the posterior part of the maxillary ramus, is in the Labyrinthodon continued to near the anterior extremity. The teeth are long and slender, gradually diminishing in size towards the anterior portion of the jaw, and the fragment presents a linear series of not less than fifty sockets, placed alternately a little more internally; and at the anterior inflected part of the jaw is the base of the socket of a large tooth. The anterior portion of the jaw being broken off, it is uncertain if the serial teeth were continued externally to the anterior tusk, a remarkable ichthyic character noticed in another species of Labyrinthodon.

The sockets of the teeth are shallower than in the upper jaw; the

[* Dr. Harlan's dissection of the *Amphiuma means* will be found in Phil. Mag., First Series, vol. lxiii. p. 325.—Edit.]

outer wall is more developed than the inner, and the anchylosed bases of the teeth more nearly resemble, in their oblique position, those of existing Batrachia. Mr. Owen then describes the microscopic structure of the teeth, and he shows that, between the apex and the part where the inflected vertical folds of the cement commence, the tooth resembles, in the simplicity of its intimate structure*, that of the entire tooth of ordinary Batrachia and most reptiles ; and in the lower or basal half of the tooth the structure described in the previous memoir commences, and gradually increases in complexity. From the long and slender character of this ramus, Mr. Owen shows that the length of the head, as compared with the breadth, approximates more nearly to Crocodilian proportions than to the ordinary Batrachian ones ; but that among existing Batrachia it resembles most nearly the Amphiume.

A dorsal vertebra from Coton End, which is next described, presents still further evidence of the Batrachian nature of the Labyrinthodon, in having concave but not deep articular cavities at the extremities of the body, a condition now known among existing reptiles only in the Gecko, and in the lower or perennibranchiate division of Batrachians. The body of the vertebra is elongate and subcompressed, with a smooth extended, but not regularly curved surface, terminating below in a slightly produced, longitudinal, median ridge ; and it exhibits the same exceptional condition in the Reptilian class as do the vertebræ of existing Batrachians, in having the superior arch or neurapophysis anchylosed with the centrum. From each side of the base of the neural arch extends obliquely, outwards and upwards, the remains of a thick and strong transverse process ; and from their strength and direction Mr. Owen gathers indications of a necessity for an expanded respiratory cavity, and that they supported ribs.

A symmetrical bone, resembling the episternum of the Ichthyosaurus, is also described. It consists of a stem or middle, which gradually thickens to the upper end, where cross-pieces are given off at right angles to the stem, and support on each a pretty deep and wide groove, indicating strongly the presence of clavicles, and thus pointing out another distinction from Crocodiles, in which clavicles are wanting.

In concluding the description of these remains of the *Labyrinthodon leptognathus*, Mr. Owen says, that they prove the fossil to have been essentially Batrachian, with striking and peculiar affinities to the higher Sauria, leading, in the form of the skull and the sculpturing of the cranial bones, to the Crocodilian group, and in one part of the dental structure, in the form of the episternum, and the biconcave vertebræ, to the Ichthyosaurus ; while in the bony palate there is a deviation from the Batrachian character, and a resemblance to the Lacertian type. Another marked peculiarity in this fossil is the anchylosis of the base of the teeth to distinct and shallow sockets, by which it is made to resemble the Sphyræna and certain other fishes. From the absence of any trace of alveoli of reserve for the

* See *antè*, p. 317.

successional teeth, Mr. Owen believes the teeth were reproduced, as in many fishes, especially the higher Chondropterygii, which formed the *Amphibiæ natantes* of Linnæus, in the soft mucous membrane which covered the alveolar margin, and subsequently became fixed to the bone by anchylosis, as in the Pike and Lophius. No remains of the locomotive organs of the *L. leptognathus* have yet been found.

Labyrinthodon pachygnathus.—In detailing the remains of this species, consisting of portions of the lower and upper jaws, an anterior frontal bone, a fractured humerus, an ilium with a great part of the acetabulum, the head of a femur, and two unguial phalanges, Mr. Owen dwells on further Batrachian characters and certain peculiarities of structure, and shows the points in which it agrees with the *L. leptognathus.* A portion, nine and a half inches long, of a right ramus of a lower jaw is first described; and in addition to the characters common to it and the fragment of the lower jaw of the *L. leptognathus,* in the structure of the angular and dentary pieces, the author shows that the outer wall of the alveolar process is not higher than the inner, as in Frogs and Toads, the Salamanders and Menopome, in all of which the base of the teeth is anchylosed to the inner side of an external alveolar plate. The smaller serial teeth are about forty in number, and gradually diminish in size as they approach both ends, but chiefly so towards the anterior part of the jaw. The sockets are close together, and the alternate ones are empty. The great laniary teeth were apparently three in each symphysis, and the length of the largest is considered to have been one and a half inch. A section through the base of the anterior tusk above the socket exhibits the structure described in Mr. Owen's first memoir; but a section of the second tusk, also taken above the socket, exhibited a very simplified modification of the labyrinthic arrangement, presenting a disposition closely analogous to that at the base of the teeth of the Ichthyosaurus. The apical half of the tusks has a smooth and polished surface, and the pulp-cavity is continued, of small size, into the centre of this part of the tooth. In the serial teeth, which in other respects, except size, correspond with the preceding description of the tusks, the central pulp-cavity is more quickly obliterated, but the alveoli are large, moderately deep and complete: the texture of the teeth is dense and brittle. The base of each tooth is anchylosed to the bottom of its socket, as in Scomberoid and Sauroid fishes; but the Labyrinthodon possesses, Mr. Owen says, a still more ichthyic character in the continuation, preserved in this specimen, of a row of small teeth anterior and external to the two or three larger tusks. A double row of teeth thus occasioned does not exist in the maxillary bones, either superior or inferior, of any Batrachian or Saurian reptile; in Mammalia it has been noticed only in the upper jaw of the hare and rabbit, and in Fishes only in the lower jaw.

A fragment of the superior maxillary bone is also described, and its chief deviation from the Crocodilian type of structure is the continuation of the palatal plate of the intermaxillary bone for about an inch to the outer side of the base of the external plate or pro-

cess; while in the Crocodiles the external wall of the intermaxillary bone is united by the whole of its outer margin with the maxillary, and is thence continued along the whole outer contour of the intermaxillary bone. Now in the Labyrinthodon the intermaxillary bone presents the same peculiar modification of the Batrachian condition of this bone as in the higher organized Batrachia, the palatal process of the intermaxillary extending beyond the outer plate both externally and, though in a less degree, internally, where it forms part of the boundary of the anterior palatal foramen, whence the outer plate rises in the form of a compressed process from a longitudinal tract in the upper part of the palatal process; it is here broken off near its margin, and the fractured surface gives the breadth of the base of the outer plate, stamping the fossil with a Batrachian character conspicuous above all the Saurian modifications by which the essential nature of the fossil appears at first sight to be marked.

In the anterior frontal bone, Mr. Owen says, there are also indications of Crocodilian structure. Its superior surface is slightly convex, and pitted with irregular impressions; and from its posterior and outer part it sends downwards a broad and slightly concave process, which the author considers the anterior boundary of the orbit. This process presents near its upper margin a deep pit, from which a groove is continued forwards; and in the corresponding orbital plate of the Crocodile there is a similar but smaller foramen.

From these remains of the cranium of the *Labyr. pachygnathus*, it is evident, Mr. Owen states, that the facial or maxillary part of the skull was formed in the main after the Crocodilian type, but with well-marked Batrachian modifications in the intermaxillary and inferior maxillary bones. The most important fact which they show is, that this Sauroid Batrachian had subterminal nostrils, leading to a wide and shallow nasal cavity, separated by a broad and almost continuous palatal flooring from the cavity of the mouth; indicating, with their horizontal position, that their posterior apertures were placed far behind the anterior or external nostrils; whereas in the air-breathing Batrachia the nasal meatus is short and vertical, and the internal apertures pierce the anterior part of the palate. Mr. Owen therefore infers that the apparatus for breathing by inspiration must have been present in the Labyrinthodon as in the Crocodile; and hence still further, that the skeleton of the Labyrinthodon will be found to be provided with well-developed ribs, and not, as in the existing Batrachia, with merely rudimentary styles. Since the essential condition of this defective state of the ribs of Batrachia is well known to be their fish-like mode of generation and necessary distention of the abdomen, Mr. Owen likewise directs attention to the probability that the generative economy of these fossil reptiles may have been similar to that of existing crocodiles.

A fragment of a vertebra presents analogous characters to the vertebra of the *L. leptognathus* previously noticed.

Of the few bones of the extremities which have come under Mr. Owen's inspection, one presents all the characteristics of the corresponding part of the humerus of a toad or frog, viz. the convex, somewhat transversely extended articular end, the internal longitudinal depression, and the well-developed deltoid ridge. The length of the fragment is two inches, and the breadth is thirteen lines. The ridges are moderately thick and compact, with a central medullary cavity. In its structure as well as in its general form, the present bone agrees with the Batrachian, and differs from the Crocodilian type.

Again, in the right ilium, about six inches in length, and in the acetabulum, there is a combination of Crocodilian and Batrachian characters. The acetabular cavity is bounded on its upper part by a produced and sharp ridge as in the frog, and not emarginate at its anterior part, as in the crocodile. Above the acetabulum in the frog the ilium gives off a broad and depressed process, the lower extremity of which is separated from the acetabulum by a smooth concave groove, both of which are wanting in the crocodile, there being only a slight rising of the upper border of the acetabulum. These characters, however, are well developed in the Labyrinthodon: but the process, instead of being depressed is compressed, and its internal extremity is pointed and bent forwards, representing the rudiment of the long anterior process of the ilium in the *Batrachia anoura*; but it does not attain in the Labyrinthodon the parallel of the anterior margin of the acetabulum, and the bone terminates in a thick truncated extremity a few lines anterior to the acetabulum; an essential feature of resemblance to the Crocodiles and difference from the Batrachians. But the most marked difference in this fossil from the crocodile is the length of the ilium posterior to the acetabulum, in which it agrees with the analogous portion of the frog and other tailless Batrachia; while, on the contrary, there is an agreement with the Crocodilian type in the mode of articulation to the vertebral column. In the frog a transverse process of a single vertebra abuts against the anterior extremity of the produced ilium. In the crocodile the transverse processes of two vertebræ are thickened and expanded, and joined to a rough, concave, articular surface occupying the inner side of the ilium, and a little posterior to the acetabular cavity. In the Labyrinthodon is a similar well-marked, rough, elongated, concave, articular surface, divided by a non-articular surface, and destined for the reception of the external extremities of two sacral ribs. The Labyrinthodon likewise agrees with the crocodile in the lower part of the acetabulum being completed by the upper extremity of the pubis, the anterior and inferior part of the ilium offering an obtuse process at the posterior part of the lower boundary of the acetabular cavity.

As the fragment of the ilium was discovered in the same quarry as the two fragments of the cranium and the portion of the lower jaws, Mr. Owen thinks they may have belonged to the same animal; and if so, as the portions of the head correspond in size with those of the head of a crocodile six or seven feet in length, but the acetabu-

lar cavity with that of a crocodile twenty-five feet in length, then the hinder extremities of the Labyrinthodon must have been of disproportionate magnitude compared with those of existing Saurians, but of approximate magnitude with some of the living anourous Batrachia. That such a reptile, of a size equal to that of the reptile whose remains have just been described, existed at the period of the new red sandstone, Mr. Owen says, is abundantly manifested by the remains of those singular impressions to which the term Cheirotherium has been applied. Other impressions, as those of the *Cheirotherium Hercules*, correspond in size with the remains of the *Labyrinthodon Salamandroides*, which have been discovered at Guy's Cliff. The head of a femur from the same quarry in which the ilium was found, is shown to correspond in size with the articular cavity of the acetabulum. The two toe-bones, or terminal phalanges, are stated to be strictly Batrachian, presenting no trace of a nail, and from their size are referred to the hind-feet of the *L. pachygnathus*.

Thus, observes Mr. Owen, all these osseous remains from the Warwick and Leamington sandstones agree in their essentially Batrachian nature, and, in this interesting conclusion, with the fossils of the German keuper; and he concludes this portion of the memoir with some observations respecting the so-called Cheirotherium footsteps. He has long believed that they were the foot-prints of a Batrachian, and most probably of that family which includes the toad and frog, on account of the difference of size in the fore and hind extremities; but, in consequence of the peculiarities of the impressions, he has always considered that the animal must have been quite distinct in the form of its feet from any known Batrachian or other reptile. Now then, he observes, we have in the Labyrinthodon also a Batrachian reptile, differing as remarkably from all known Batrachia and from every other reptile in the structure of its teeth: both the footsteps and the fossils are, moreover, peculiar to the new red sandstone; and though the generic name Labyrinthodon may be susceptible hereafter of being expanded to the appellation of a family, yet, he asks, may it not be justifiable to consider the term Cheirotherium as one of the synonyms of Labyrinthodon?

Labyrinthodon scutulatus.—The remains, to which this specific designation has been applied by the author, composed a closely and irregularly aggregated group of bones imbedded in sandstone, and manifestly belonging to the same skeleton; they consist of four vertebræ, portions of ribs, a humerus, a femur, two tibiæ, one end of a large flat bone, and several small osseous, dermal scuta. The mass was discovered in the new red sandstone at Leamington, and was transmitted to Mr. Owen by Dr. Lloyd in the summer of 1840.

The vertebræ present biconcave articular surfaces similar to those of the other species. In two of them, the surfaces slope in a parallel direction obliquely from the axis of the vertebræ, as in the dorsal vertebræ of the frog, indicating an habitual inflexion of the spine, analogous to that in the humped back of the frog. The neurapophyses are anchylosed to the vertebral body. The spinous process rises from the whole length of the middle line of the neurapophysial

arch, and its chief peculiarity is the expansion of its elongated summit into a horizontally flattened plate, sculptured irrregularly on the upper surface. A similar flattening of the summit of the elongated spine is exhibited in the large atlas of the toad. The body of the vertebræ agrees with that of the *L. leptognathus*. The humerus is an inch long, regularly convex at the proximal extremity, and expanded at both extremities, but contracted in the middle. A portion of a somewhat shorter and flatter bone is bent at a subacute angle with the distal extremity, and resembles most nearly the anchylosed radius and ulna of the Batrachia.

The femur wants both the extremities; its shaft is subtrihedral and slightly bent, and its walls are thin and compact, including a large medullary cavity. The tibiæ are as long, but thicker and stronger than the femur. They had lost their articular extremities, but exhibited that remarkable compression of their distal portion which characterizes the corresponding bone in the Batrachia: they likewise have the longitudinal impression along the middle of the flattened surface. The length of the more perfect shaft is 2 inches 1 line. The precise nature of the broad flat bone, Mr. Owen had not determined.

With respect to the osseous dermal scuta, Mr. Owen remarks, that though they form a striking instance of the Crocodilian affinities of the Leamington fossil, yet as these detached superficial bones are the most liable to be separated from the fragmentary skeleton of the individual they once clothed, the negative fact of their not having been found associated with the remains of the Labyrinthodon in other localities proves nothing in regard to a difference of dermal structure between the Leamington and Warwick species. Indeed no anatomist, he says, can contemplate the extensive development and bold sculpturing of the dermal surface of cranial bones in the *Labyrinthodon pachygnathus* and *L. leptognathus* without a suspicion, that the same character may have been manifested in bony plates of the skin in other parts of the body. Admitting for a moment this structure to be proved, to what extent, asks Mr. Owen, does it affect the claims of the Labyrinthodon to be admitted into the order of Batrachians, in which every known species is covered with a soft, lubricous and naked integument, without scales or scutæ? In reply, he says, that the skin is the seat of variable characters in all animals; and, apart from the modifications of the osseous and dental systems, and other intimate organs, is apt to mislead the naturalist who is in quest of the real affinities of a species: and he instances the Trionyx, as an example of a soft-skinned animal among Chelonian reptiles. Lastly, Mr. Owen shows, that, previously to the discovery of the fossils described in this memoir, the only Batrachian remain which had been found in beds anterior to the epoch of the Molasse is the fragment of a skull, on which Prof. Jaëger founded his *Salamandroides giganteus*.

March 10.—A paper "On the Geological Structure of the Northern and Central Regions of Russia in Europe," by Roderick Impey Murchison, Esq., P.G.S., and M. E. de Verneuil, V.P.G.S. of France, was commenced.

ZOOLOGICAL SOCIETY.

Feb. 9.—A continuation of the descriptions of Mr. Cuming's shells, by W. J. Broderip, Esq., was read, for which we refer to No. 97 of the Society's Proceedings. The following remarks by the author, and letter from Sir D. Brewster, were appended to the description of *Bulinus velatus*.

In a great number of the beautiful land-shells of the Philippine Islands, collected by Mr. Cuming, and herein-before described, the pattern, upon immersion in water or other fluid, becomes entirely obliterated till evaporation restores the colours to all their pristine brilliancy. In the species now before us, the very reverse is the result of immersion. The external whitish porous epidermis which veils the shell when dry, suffers the bright colours to shine out when immersed in water. *Bul. velatus* is described above, as it appears on immersion, and before it becomes dry: but in the latter state the beauties of the shell are shrouded, and the colour of the sutural bands, peeping out between interstices in the epidermis, gives to these bands a moniliform appearance.

I sent to Sir David Brewster, as the highest authority on such subjects, four or five species of those land-shells from which the pattern disappears upon immersion; but I have not as yet forwarded to him any upon which the colours come out when so treated. Sir David has been so obliging as to send me the following letter, which I now lay before the Society:—

"Dear Sir,—I beg to return you my best thanks for the very interesting specimens of land-shells from the Philippine Islands, which you have been so kind as to send me. The disappearance of the white pattern by immersion in water or any other evaporable fluid, and its subsequent reappearance when the shell is dry, are phenomena perfectly analogous to those of *hydrophanous opal, tabasheer,* and other porous substances.

"The phenomenon in the land-shells is still more beautiful when we examine them by *transmitted* light. The pattern which is *white* by *reflected* light, is *dark* by *transmitted* light, and *vice versâ*. This is particularly beautiful in the *Helix pulcherrima,* where the ground of the *white* pattern is almost *black* by reflected light, and of a light *reddish* colour by transmitted light, the pattern which is *white* by reflection having a *dark red* colour by transmitted light.

"In all these shells, the difference of structure by which the pattern is produced, does not exist in the shell, but in the *epidermis,* and hence the pattern may be wholly obliterated by removing the epidermis. It appears to me, from very careful observations, that the epidermis consists of two layers, and that it is only the upper layer which is porous wherever the pattern is white. These *white* or porous portions of the epidermis differ from the other parts of the upper layer only in having been deprived of, or in never having possessed, the element which gives transparency to the membrane, in the same manner as hydrophanous opal has become *white,* from the expulsion of its water of crystallization.

"When the shell is immersed in water or any other fluid, the fluid enters the pores of the white epidermis, and having nearly the same

refractive power as the epidermis, no light is reflected at the separating surface of the water and the pores which contain it, so that the light passes through the membrane, which thus loses its white appearance. When the water escapes from the pores by evaporation, or is driven from them by heat, the membrane again reflects white light from the numerous surfaces of its pores.

"As the colouring matter resides in the shell itself, its peculiar colour is seen through the epidermis as distinctly where it is porous as where it is not porous, when the porous portion has been rendered transparent by the absorption of a fluid.

"If we apply oil or varnish to the white pattern, we may obliterate it permanently, or we may change it into a pattern entirely different from the original one.

"If these observations appear to you to have any interest, you are welcome to make any use of them you please.

"I am, dear Sir, ever most faithfully yours,

"D. Brewster."

It will be observed, that Sir David Brewster points out how the application of oil or varnish to the white pattern may obliterate it permanently; such a case has already happened more than once: persons who have become possessed of some of the species whose patterns are lost on immersion, not content with their natural beauties, and unaware of their peculiarity, have had recourse to art, and by applying oil or varnish, have spoiled their specimens,—a proper punishment for trying to mend nature.

LONDON ELECTRICAL SOCIETY.

Oct. 19, 1841.—A Translation by the Secretary, of "Observations on the Electrical Effects of the Gymnotus," by Professor Schœnbein, was read.

The author having related at some length the nature and results of certain experiments he made with the Gymnotus of the Adelaide Gallery, proceeds to examine the reasons which have induced some philosophers to attribute its electrical powers to the peculiar physical or mechanical construction of the electric organ, and adduces a mass of facts in proof that this organ bears very little analogy to Volta's pile. For instance, it consists of a combination of substances possessing naturally but a very feeble electromotive power, and yet the fish will give shocks equal in force to those obtained from a very large Leyden jar charged at its maximum, or from 200 pairs of a highly excited voltaic battery. Again, the creature dwells in a conducting medium which completes at all times the circuit between the poles of its organ, and yet no passage of electricity occurs but at its will; it is precisely as if it possessed a means of insulating or uninsulating the apparatus at pleasure; although physiologists have not detected any arrangement fitted for such a purpose. The fish too can regulate the intensity of its discharges at will; this is especially evident when a comparison is made between the effects produced by touching it with the human hand, or through the medium of a metallic conductor. The power evidently depends upon an intimate union, of which we know not

the nature, between the vital powers and the physical functions of this organ; for, like all vital organization, the power is exhausted by repeated efforts.

"An Account of Experiments with a Water Battery." By H. M. Noad, Esq., Memb. Elect. Soc.

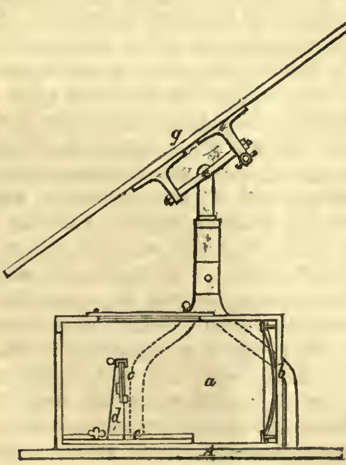
The water battery employed by the author of this paper consisted of 500 pairs, and produced the effects which are known to bear so close an analogy with those from the electrical machine. Among the results, the deposition of carbon, when the electrodes were placed in the flame of a candle, was very characteristic of the peculiar action under these circumstances. The nature of the deposit was first noticed by M. Gassiot.

The Secretary then laid before the Society Mr. Weekes's Monthly Register of the electrical condition of the atmosphere for September.

LXI. *Intelligence and Miscellaneous Articles.*

NOTICE OF BEARD'S PATENT FOR IMPROVEMENTS IN APPARATUS FOR OBTAINING LIKENESSES AND REPRESENTATIONS OF NATURE, &c.

THE drawing shows various views of the apparatus, the nature of which is written on the drawing, and the same parts are referred to by the same letters of reference. *a* is a rectangular box, inside of which and at the end thereof, there is fixed a concave reflector, which may be of glass or metal, as is well understood in making reflectors for telescopes. This reflector is placed with the reflecting surface facing the other end of the box, which has an opening corresponding to the size of the reflector. *c* is a light frame fixed by a support, *d*, to a piece of wood or other material, *e*, with which it slides on the bottom of the box in a direction to and from the face of the reflector and lengthwise of the box; this frame is intended to carry the prepared material on which the impression is to be made

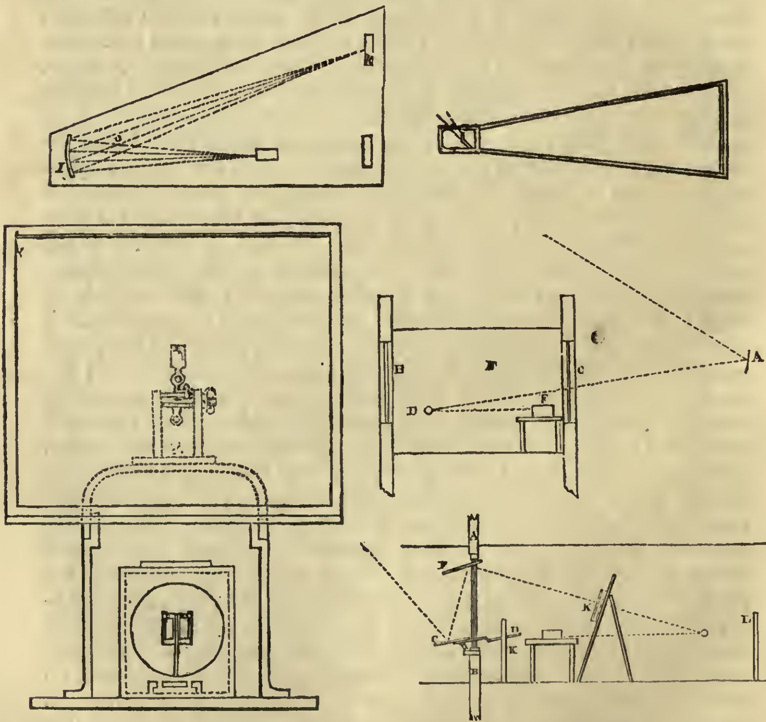


by the reflected image; the material may be retained in the proper position against the frame by a small spring, *f*, pressing against the material on the back; and between the spring and the frame, *c*, the prepared surface is slid. A small door is made on the top of the box for the purpose of observing the focal image, and the frame is slided till properly adjusted to the focus. The box, *a*, should be placed on a table or other support, at such height that the centre of the reflector may be about as high as that part of the person which is intended to be in the middle of the picture. When the reflecting apparatus is to be used, the person whose likeness is to be taken should be placed in a chair, to which some suitable support for the head is attached, to enable him to remain perfectly still, and the reflecting aparatus should then be placed with the open end immediately opposite to the person. A trial surface is then to be put against the frame to receive the reflected image, by which means its correct placing will readily be judged of by looking through the hole in the box, *a*, and the focus will be adjusted by sliding the piece, *e*; the trial surface is then to be removed, and the prepared surface is to be placed in the frame and allowed to remain as long as required to form the image, and a little practice will readily enable the operator to judge when the required effect is obtained. The size of the reflecting apparatus I use, is as follows: the box, *a*, inside fifteen inches long, eight and a half inches high, and eight inches wide, and the interior is black; reflector seven inches clear diameter, and twelve inches focus; the prepared surface on to which the picture is to be formed is two and a half inches long, by two inches wide, but these dimensions may be varied. In using the apparatus above described, when the daylight is very bright, I prefer that the inclined glass roof, A, of the room should be glazed with blue glass, or otherwise to soften the bright rays of light, in order that the person sitting to have a likeness taken may be as near as possible to the glass roof; but when there is a bright sunshine, I use a large looking-glass or reflector. I prefer to use a large concave reflector to collect the rays of light from the sun and throw it on the person sitting.

I place the reflecting apparatus out of the centre of the large reflector, B, generally behind the large reflector, by having a hole through such large reflector towards the outer edge thereof; and in making such large reflector, B, I find that it may be done by forming a frame of wood to the figure or shape required, and then covering the surface with small squares of looking-glass of five or six inches square; this is a cheap and convenient way of making such a reflector on a large size for the purpose, but I do not confine myself thereto, the object being to obtain a large reflector to collect the light and direct it strongly on a person or other object, from which it is desired to obtain a likeness or image on to a prepared surface. In preparing plates with silver surfaces to render them more suitable for receiving impressions or images thereon, and then have the surface protected from change, I take sheets of copper with a surface of silver, such as is now commonly used according to the process known as Daguerreotype, and place two such plates together, with their silver surfaces touching, those surfaces having been care-

fully wiped clean with cotton, and aided, if necessary, with dilute sulphuric acid; and I pass such two plates five or six times between a pair of smooth hardened rollers. I then anneal the plates by heating them to a low red heat and permit them to cool. Then I again subject them to the acid process, and then I pass them, silver face to face, between the rollers, the rollers being set slightly closer together as the plates will have become somewhat thinner, and I repeat such rolling and annealing till I find the silver surfaces are highly polished and equal in appearance all over the silver surfaces. They will then be ready for the next process, which consists in taking a small bunch or tuft of cotton, and dipping it into dilute nitric acid, and then into tripoli, and very lightly rubbing the silver surface of a plate, then quickly and carefully rubbing off the acid and tripoli with dry cotton: in these rubbings a circular motion is to be observed. I then take a surface of velvet, having dusted on it some impalpable powder of charcoal from a muslin bag, and rub the silver surface of the plate in a direction transversely of the length of plate, when the required surface will be obtained, and is ready to undergo the iodine process, as is well understood; but I prefer that the iodine should not be used separately, but that it should be combined with nitric acid and water, or with bromine, or with both, or with bromic acid; and I perform this operation in the following manner: I place a square glass vessel somewhat larger than the plate to be operated on, in a box made of wood, with a cover with an opening at each end to allow of a plate of glass to slide closely across, so as to permit of as little escape of vapour as possible, the plate glass slide being somewhat more than twice the length of the box, and in one part thereof, towards one end, an opening is formed through the glass large enough to receive the metal plate and yet not to allow it to drop through; by this means the slide can bring the plate of metal directly over the box, in order that the vapours as they arise may come in contact with the silver surface, which is placed downwards, and in a few seconds it will be ready to be put into the reflecting apparatus, to receive an impression, or it may be put into a suitable dark case for holding several such plates ready for use. In combining iodine with nitric acid and water, I put equal parts of iodine, nitric acid, and of water, and in combining bromine therewith, I combine an equal part with each of the other three, or I omit to use nitric acid, and use sulphuric acid and water, or I omit the use of acid and simply combine iodine and bromine in the box above mentioned, and the required vapours will quickly operate on the metal plate, the other portion of the glass slide closing the box, when no plate is being operated on. And I have only further to remark, that when the impression has been obtained, the plate is to be operated on by the mercury, and the fixing and washing processes, in like manner to the treatment well understood when treating plates which have, according to the process of Daguerreotype, received an impression or image by the aid of a camera, all which is well known, and in use, and forms no part of the invention. In taking impressions or obtaining images by the aid of the apparatus above described, it is desirable to use a screen behind the person sitting to have a likeness taken, and for this purpose a plate of ground

plate glass or other semi-transparent surface, through which the shadow of the person may pass; and at the back of such screen I have a frame capable of being set nearer to or further from the back of the screen, and in this frame I have a white surface capable of receiving the shadow so passed through the semi-transparent screen, and by causing such white surface to come nearer to or further from the screen, I obtain varying effects of light and shade in the picture produced. And I find it of advantage sometimes to use a brown, blue, or black back surface, particularly when taking images of plaster busts or other white objects; for it will be evident that although I have thus far confined the description to the taking likenesses from human beings, any other object having the apparatus placed suitably in front of it, will be received on the reflector, and reflected to the prepared surface, in like manner to that of a person. And I would also remark, that in using a large concave reflector to collect the light as above explained, I find it desirable to stretch across the front thereof, a surface of tissue paper varnished with boiled oil, such as is used for tracing paper, as it has the effect of scattering the light and producing a better effect in the picture.



Repertory of Patent Inventions, N.S., vol. xv. p. 13.

ON THE POLISHED ROCKS OF FONTAINBLEAU. BY M. DUROCHER.

In the *Comptes Rendus de l'Académie des Sciences,* for July 12, 1841, we find a "Notice on the traces of polishing which the diluvium has left on the sandstones of Fontainbleau," by M. Durocher, of which the following is a translation.

In the places where the diluvium of the valley of the Seine has extended, following the general direction from east-south-east to west-north-west, those marks of polish which are seen so frequently and so distinctly in Scandinavia and in the Alps, had not hitherto been observed on the surface of the rocks. This is probably occasioned by the soft nature of the rocks which form the Paris basin, and their mechanical disaggregation under the influence of exterior agents. It is the same in the countries to the south of the Baltic, on which the diluvium of the north has formed thick deposits of the fragments of the Scandinavian rocks: as only schists or limestones are found in them, it is not possible that the marks produced by the passing of the currents should have been preserved to our times; and yet their existence cannot be called in question; for in boring a well through the diluvial soil in Prussia, on reaching the solid rock it was found polished and grooved as in Sweden and Finland: the deposit which covered it had served as the means of preservation. The absence of these traces in many places which have been the theatre of diluvial phænomena, is therefore to be attributed to atmospheric action.

Amongst the formations which constitute the Paris basin, the sandstones alone appear to me capable of preserving any remains of diluvial impressions: in an excursion which I recently made to Fontainbleau, I indeed recognised that these marks were not entirely effaced. In the forest of Fontainbleau there are but very few spots where this can be observed; the principal cause which hinders this, is, that the solid sandstone seldom appears at the surface in great areas. It is almost everywhere covered with a vegetable crust, and unfortunately in the small number of cases where it may be seen, it is nearly always disaggregated and reduced to sand. As to the blocks which are found heaped up in such great numbers and prodigious size, it is difficult to discern upon their surface the marks of the currents of water which displaced them, and gave them their various and fantastical forms. It is only in the spot well known by the name of the *Gorges de Franchard,* that I was clearly able to recognise the traces of the passage of great masses of water.

I will endeavour to notify, as precisely as possible, the points where I observed it most distinctly. In following the road which leads from the Cross of Souvray (on the road of Ury) to Franchard, when we reach the spot where this road makes a slight deviation to the north-east, we must take the road which leads to the Rochers-des-hautes-plaines, and turn at the third path to the right: this crosses a small plain covered with heath, and leads straight to the defile of Franchard. It opens into the principal valley by a slightly inclined

entrance, and forms, as it were, a cutting in the line of rocks which border the valley on this side. It is by descending this way, that the traces of polish may be observed on both sides of this species of defile, but chiefly on the right side. There are very large blocks there, and also masses of flattened rock, which seem to be a part of the sandstone layers, or to have been but little moved from their position. Upon examining their surface, we see that it is polished, and even presents indications of broad furrows, the resemblance of which to the diluvian furrows of the Alps or of Scandinavia, it is impossible to mistake. A circumstance which precludes any doubt that the marks are owing to the action of masses of water, is, that they are directed according to the declivity of the defile, and fall towards the interior of the central valley.

Upon examining the surface quite near, we see no fine grooves as we do on the granites and gneiss of the Alps; nor is there any reason to be surprised at this, because these sandstones, not being formed of grains, which are strongly agglutinated together, were not adapted for receiving and retaining very fine grooves. The polish is also visible on the largest blocks of sandstone; there are several flattened, which are rather sunk in the earth or lie above it on their broadest side, which is nearly horizontal and rugged, whilst the superior surface is polished; by a close examination, we find that the polish is not owing to the friction of these masses against each other or against the earth, but to the action of the waters.

On the north anticlinal of the valley of Franchard, on the side opposite to that I have just mentioned, we find another little defile at the spot where the mass of blocks called Roche-qui-pleure is situated: there the polish is still visible, but only upon some blocks having the position already described; several amongst them show on their sides circular and rounded cavities, which appear to me to have been caused by a whirling of waters; they seem to have followed the declivity of the land in its present configuration, and to have been precipitated towards the middle of the valley. It would have been interesting to see the disposition of the traces at the very bottom of the valley; but it is impossible, for the ground is covered with sand and vegetable earth: there are very few blocks; the greater number of them are in heaps, crowning the two crests.

This valley of Franchard may be compared to a cutting hollowed by the waters in the high plain of sandstone which forms the western part of the forest, and the level of which is very much higher than that of the town of Fontainbleau. The mean direction is from the east 10° south, to the west 10° north; this is the general direction of the numerous lines of hills covered with blocks, and disposed in the form of bands extended in the same direction; the most prominent type of this disposition is the small chain which extends from the Sablons to near Milly, and the different rings (*anneaux*) of which bear the names of Rocher-de-bon-Ligne, of the Salamander, of Trape-charette, &c.

As M. Elie de Beaumont has so judiciously observed, these bands take the direction W.N.W., which the Seine follows before recei-

ving the Loing at Saint Manvert, and which it again takes beyond Melun. It is probable that at the diluvian epoch the current followed the same line from the E.S.E. to the W.N.W., that it must have covered the whole extent now occupied by the forest, and that it hollowed out those broad furrows, the ridges of which are crowned by blocks. The sandstone of Fontainbleau being formed of friable and of more cohering layers, the former were doubtless undermined by degrees by the action of the waters; and then the solid layers, being no longer upheld at their base, must have been broken up, and have produced those vast heaps which give such wild and picturesque aspects to this forest.

[On the subject of the preceding notice of M. Durocher, see abstracts of communications made to the Geological Society by Prof. Agassiz, Dr. Buckland, and Mr. Lyell, p. 565, 569—590, of the preceding volume (S. 3. xviii.) of the Philosophical Magazine; and some remarks by Mr. Murchison and M. de Verneuil, in their paper on the Geology of Russia, a notice of which will appear in a future Number: also Dr. Locke's notice of diluvial or glacial scratches on rocks in America, noticed in the Annals and Magazine of Natural History, vol. vii. p. 524; and M. Sefström's Memoir on the Furrows of the Scandinavian Mountains in the Scientific Memoirs, Part ix. —Edit.]

ON THE COMPOSITION OF THE BRAIN OF MAN. BY M. FREMY.

It results from the researches of M. Fremy that the brain of man is formed of a considerable quantity of water and of matter insoluble in æther, which he describes as albuminous matter.

The portion soluble in æther is formed chiefly of three substances:

1st. The white matter discovered by Vauquelin, in which M. Fremy has detected very decided acid properties, and which he calls cerebric acid.

2ndly. A liquid fatty matter, which has all the properties and composition of the olein of human fat, analysed by M. Chevreul.

3rdly. Cholestrin discovered in the brain by M. Couerbe. There are besides found in the brain variable and very small quantities of oleic acid, margaric acid, cerebrate of soda, and albuminous matter; to obtain these results, the author cuts the brain into small pieces, boils it repeatedly in alcohol, and allows it to remain for some days in this liquid. The object of this operation is to remove the water contained in the brain, and to coagulate the albumen; the cerebral mass has then lost its elasticity, and may be submitted to pressure; the alcohol retains traces only of cerebric acid, which may be separated by the filter; the brain is then to be treated with boiling alcohol, till it ceases to dissolve any further portion; the liquors are to be evaporated, and the residue treated with boiling absolute alcohol, which removes the olein, the cerebric acid, the cholestrin and the oleic and margaric acids; the albuminous matter and cerebrate of soda do not dissolve. On the cooling of the alcohol the cholestrin and cerebric acid are deposited; these are to be separated by cold æther, which readily dissolves the cholestrin and leaves the cerebric

acid. In order to purify the cerebric acid, it must be boiled with slightly acidulated alcohol, in order to decompose the cerebrate of soda, which is always mixed with cerebric acid. Cold alcohol holds the olein, and the oleic and margaric acids: it is to be rendered slightly alkaline by ammonia and evaporated, and by these means the olein is deposited when the liquor has acquired a certain degree of concentration, whilst the oleate and margarate of ammonia remain in solution.

The albuminous matter and the cerebrate of soda, which have resisted the action of the absolute alcohol, are in their turn treated with boiling alcohol, acidulated with hydrochloric acid, which decomposes the cerebrate of soda. The cerebric acid set free dissolves very readily in alcohol; there then remains a coloured matter, albuminous in its nature, which contains sulphur, but never phosphorus.

After having thus determined the composition of the fatty matters of the brain, M. Fremy examined the substances which M. Couerbe has described by the names éléencéphol, céphalote and stéaroconote: and he states that the first is formed of olein and cerebrate of soda; that the second contains olein and cerebrate of soda, associated with traces of albumen; and that the last is merely a mixture of albumen and cerebrate of soda.

In analysing the brain in different states and of different ages, M. Fremy found that the quantity of free fatty acids was variable; and that it even sometimes increased when the fatty matters were left in a closed bottle. He discovered the cause of this curious phænomenon by referring to the observations of M. Chevreul on the fat of carcases, and to the memoir of MM. Pelouze and Boudet, in which they mention the spontaneous saponification of palm oil. He further observed, that it was the albuminous matter of the brain which had the property of eventually converting the olein into oleic acid. Lastly, he found by analysis, that all the fatty bodies occur in the white substance of the brain, and that the gray portion contained only traces of it; if therefore the anatomy of the brain were to be represented in a chemical point of view, it may be said that the substance which forms, so to speak, the frame of the brain, is originally gray, and that it is the fatty matter which infiltrates and spreads in the interior of this gray matter, that forms the white zones which constitute the white portion of the brain.—*Journal de Pharmacie*, tom. xxvii. p. 766.

ACTION OF PEROXIDE OF IRON ON POTASH. BY M. FREMY.

The evident analogy which exists between iron and manganese, leads to the opinion that similar compounds of them will sooner or later be formed.

This idea induced M. Fremy to suppose that it might be possible to form salts in which an oxygenated compound of iron would act the electro-negative part, and which would thus correspond with the compounds of manganic and oxymanganic acid with bases; and the following experiments are conceived by M. Fremy to demonstrate that iron can give rise to compounds produced under the same circumstances as the manganates, and possessing a striking analogy with that species of salts.

When a mixture of peroxide of iron and potash is kept for some time at a strong red heat, a brown mass is obtained, which, treated with water, gives a solution of a very fine violet-red colour; this compound is very soluble in water, but a large quantity of water eventually decomposes it: it becomes insoluble in water which is very alkaline, but gives a brown precipitate, which dissolves readily in pure water, and yields a solution of a fine purple colour. It appears to be much less permanent than manganate of potash; under certain circumstances it decomposes at common temperatures into oxygen gas, which escapes, and peroxide of iron which precipitates; and a solution of free potash remains, which is perfectly colourless. At 212° Fahr. it undergoes similar decomposition instantaneously: all organic substances decompose it; consequently it is impossible to filter the solution.

This compound may be more readily prepared, and in a few minutes, by calcining, at a high temperature, a mixture of nitre, potash and peroxide of iron; or by heating a mixture of peroxide of potassium and peroxide of iron. This compound was also formed in the humid way by passing a current of chlorine gas into a very concentrated solution of potash, holding peroxide of iron in suspension.

The fact stated seems to indicate the existence of a body more highly oxygenated then peroxide of iron. M. Fremy could not, however, isolate any such compound; for when the solution is treated with an acid, and the potash is saturated, oxygen is disengaged and peroxide of iron is precipitated; if the acid is in excess, the peroxide of iron is dissolved, and a salt of this oxide is formed.—*Journal de Pharmacie*, tom. xxvii. p. 97.

PREPARATION OF GUAIAIC ACID. BY M. THIERRY.

To prepare this acid M. Thierry dissolves the pure guaiacum resin of commerce in the requisite of alcohol, sp. gr. (56 c.); the tincture is to be filtered, and three-fourths of the spirit used are to be distilled; when the operation is over there remains in the retort a yellowish liquor, above a quantity of resin; when cool the liquor is to be filtered.

This liquor is acid, and is to be saturated with barytes water, with which it forms a soluble salt; the liquor is to be evaporated to half in a water-bath, and the guaiacate obtained is to be decomposed by dilute sulphuric acid; and it is better that a little of the salt should remain undecomposed, rather than excess of acid should be used; for the latter during the concentration of the guaiaic acid reacts upon and decomposes a portion of it; if, however, any should have been employed, a little barytes water may be used to precipitate it; when the sulphate of barytes has been separated the solution is to be evaporated in a water-bath to the consistence of a syrup; the residue is to be put into a matrass, and the mixture is to be shaken repeatedly for some time; the æther dissolves the guaiaic acid, but does not act upon the extractive matter; the liquor is to be decanted, and when the æther is evaporated, the guaiaic acid is deposited in irregular mammillated masses on the sides of the vessel. The acid thus obtained is not however quite pure; it contains some

resinous matter, and in order to free it from this it must be sublimed very cautiously.

This acid is obtained in the state of fine needles; it is soluble in water, in alcohol and in æther; it differs from benzoic and cinnamic acid in their being slightly soluble in water, whereas guaiaic acid is perfectly soluble in it.

M. Thierry has not yet analysed this acid, and he states that the substance noticed by M. Righini as guaiaic acid in the *Journal de Chémie Médicale,* 1836, is not truly this acid, but a resinous matter which has been saponified by magnesia, and which is precipitated when no longer held in solution by an alkali.—*Journal de Pharmacie,* tom. xxvii. p. 382.

LONDON INSTITUTION.

The Lectures for the season will commence on Monday, November 8th, when Mr. Grove (whose appointment to the Chair of Experimental Philosophy in this Institution we have already recorded) will deliver the first lecture of a Course on *Magnetism.* On Thursday, November 11, Mr. Brayley will begin a Course on *Meteorology*; and on Thursday, January, 6, 1842, Professor Grove will commence another Course, the subject of which will be *the Physical Elements of the Ancient Philosophers.* The series will also include lectures *on Manufactures,* by Mr. E. Cowper, of King's College; *on Rome,* by Dr. Vaughan; *on Painting,* by Mr. Haydon; *on Music,* by Mr. Gauntlett; *on the Fossil Remains of Extinct Animals,* by Professor Ansted, of King's College; *on Numismatics,* by Mr. J. Williams; *on America,* by Mr. Buckingham; and *on Shakspere,* by Mr. C. C. Clarke. Four Conversazioni will also be held, on the evenings of Wednesdays, January 19, 1842, February 16, March 16, and April 20.

Mr. Brayley's Course on METEOROLOGY will consist of Four Lectures; designed, principally, to illustrate the more recent observations and discoveries, and to review the present state of our knowledge respecting Igneous Meteors and Meteorites.

SCIENTIFIC BOOKS.

CONCHOLOGIA SYSTEMATICA, or complete System of Conchology: in which the Lepades and Mollusca are described and classified according to their Natural Organization and Habits; illustrated with 300 highly finished Copper Plate Engravings, by Messrs. SOWERBY, containing above 1500 figures of Shells. By LOVELL REEVE, F.Z.S., Member of the Cuvierian Society of Paris, &c.

The UNDULATORY THEORY, as applied to the Dispersion of Light. By the Rev. BADEN POWELL, M.A., F.R.S., F.G.S., Savilian Professor in the University of Oxford.

A MANUAL OF ELECTRICITY, MAGNETISM AND METEOROLOGY. By DIONYSIUS LARDNER, D.C.L., F.R.S., &c.

A LIST of the GENERA of BIRDS, with their Synonyma, and an indication of the typical species of each genus, by GEORGE ROBERT GRAY. 2nd edit. 8vo.

A New Process for purifying the Waters supplied to the Metro-

polis by the existing Water Companies, rendering each water much softer, preventing a fur on boiling, separating vegetating and colouring matter, destroying numerous water-insects, and withdrawing from solution large quantities of solid matter, not separable by mere filtration. By Thomas Clark, Professor of Chemistry in the University of Aberdeen. 2nd edit. 8vo.

An Illustrated Catalogue of British Plants, arranged according to the Natural Orders, with references to Smith, Hooker, Lindley, Macreight, &c. By C. E. Sowerby, A.L.S. To be continued monthly.

An Essay on Single Vision. By John Thomas Woodhouse, M.D., Fellow of Gonville and Caius College, Cambridge.

A Practical Treatise on the Manufacture and Distribution of Coal-Gas; its Introduction and progressive Improvement; illustrated by Engravings from working drawings, with general estimates. By Samuel Clegg, Jun., Civil Engineer.

Preparing for Publication.

The Geology and Mineralogy of Engineering; comprehending the elements of the sciences of Œconomic Geology and Mineralogy applied to the Arts. By E. W. Brayley, Jun., Fellow of the Linnæan and Geological Societies; Associate of the Institution of Civil Engineers; Corresponding Member of the Royal Geological Society of Cornwall, &c.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1841.

Chiswick.—Sept. 1. Very fine. 2. Slight fog: very fine: clear. 3. Very fine: showery: heavy rain. 4. Stormy and wet. 5. Cloudy and fine: rain: cold fog. 6. Dense fog: hazy: foggy at night. 7. Foggy: rain. 8. Fine. 9. Overcast. 10, 11. Foggy: very fine. 12, 13. Very hot for the period of the season. 14. Dry haze: very fine. 15. Very fine: rain at night. 16—20. Very fine. 21. Hazy: very fine. 22. Rain: very fine. 23. Heavy rain. 24. Cloudy: rain. 25. Showery. 26. Showery: stormy with rain at night. 27. Fine: lightning and very heavy rain at night. 28. Rain: boisterous. 29. Boisterous: clear at night. 30. Boisterous, with rain: clear and fine.

Boston.—Sept. 1. Foggy: rain yesterday P.M. 2. Fine. 3. Rain; heavy rain P.M. 4. Cloudy: stormy, with rain P.M. 5. Cloudy: rain P.M. 6. Fine. 7. Cloudy: rain P.M. 8. Cloudy. 9. Fine. 10. Cloudy: thermometer 74° three o'clock. 11. Cloudy. 12. Fine: thermometer 80° half-past eleven o'clock A.M. 13. Fine: thermometer 74° three o'clock P.M. 14. Fine. 15. Cloudy. 16. Fine: rain A.M. 17. Fine. 18. Foggy. 19—21. Cloudy. 22. Cloudy: rain A.M. 23. Rain A.M. 24. Cloudy: rain early A.M. 25. Fine. 26. Cloudy: rain early A.M.: rain P.M. 27. Fine. 28. Stormy: rain early A.M. 29. Stormy. 30. Cloudy: rain early A.M.: rain P.M.

Applegarth Manse, Dumfries-shire.—Sept. 1. Fair till P.M., then rained. 2. Continued rain P.M. 3. Fair and fine. 4. Fair and fine, but cloudy A.M. 5. Fair and fine. 6. Fair and fine: hoar-frost A.M. 7. Cloudy A.M: rain P.M. 8. Wet A.M.: cleared up. 9. Wet nearly all day. 10. Wet throughout. 11. Cloudy A.M.: wet P.M. 12. Hot sun: fiery wind: thunder. 13. Clear: fiery wind: thunder. 14. Showery A.M.: wet P.M.: thunder. 15. Cloudy, but fair: thunder. 16. One shower. 17. Fair and clear. 18, 19. Fair and fine. 20, 21. Fair and fine, though windy. 22. Shower in the afternoon. 23. Fair and fine. 24, 25. Showers. 26. Wet A.M.: cleared and was fine. 27. Fair but threatening. 28. Heavy showers. 29. Heavy rain all day: thunder. 30. One or two slight showers.

Sun shone out 26 days. Rain fell 15 days. Thunder 5 days. Frost, hoar, 2 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Sept.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
○ 1.	30·068	30·092	29·991	29·49	29·78	29·68	59·7	72·8	52·7	72	36	53	64	40½	NW.	S.	calm	S.	...	...	·23	...	59
2.	29·896	29·843	29·752	29·37	29·55	29·49	60·2	67·7	51·3	73	48	57	60	53	E.	S.	calm	S.	...	...	...	...	57
3.	29·744	29·691	29·439	29·20	29·57	29·60	65·3	69·8	54·7	72	50	56	59½	49	SSE.	SW.	calm	WNW.	...	·48	·10	...	58
4.	29·546	29·775	29·444	28·88	29·58	29·61	54·7	73·4	52·7	55	41	60·5	54	46½	W var.	W.	SW.	NNE.	·536	·32	·84	...	57
5.	29·874	29·823	29·753	29·36	29·68	29·68	53·2	57·6	47·2	56	36	51	59	36	S.	E.	calm	NW.	·402	·04	·52	...	54
6.	29·762	29·725	29·704	29·17	29·65	29·63	52·5	56·3	42·8	61	37	48	58	33½	S.	S.	calm	SSW.	·019	...	·15	...	49
7.	29·800	29·734	29·582	29·31	29·51	29·37	55·7	58·7	45·8	62	49	50	56	34½	S.	S.	calm	ESE.	...	·17	...	0·37	50
☾ 8.	29·800	29·951	29·717	29·16	29·45	29·74	60·7	62·0	52·6	70	51	60	58½	48½	W.	SW.	W.	WSW.	·183	...	·13	...	54
9.	30·076	30·019	29·959	29·40	29·80	29·70	59·3	68·3	56·2	68	56	60	61	39½	E.	S.	calm	SE.	...	...	...	...	55
10.	30·066	30·002	29·985	29·31	29·73	29·71	63·8	67·4	59·2	74	55	64·5	61	56	WSW.	SW.	calm	SE.	...	...	...	...	59
11.	30·122	30·052	29·976	29·47	29·81	29·88	59·8	69·4	57·2	78	54	60	61½	55	W.	NE.	calm	SSE.	...	...	...	...	60
12.	30·012	29·923	29·598	29·36	29·80	29·75	68·4	74·7	60·6	84	56	70	81	55	E.	SE.	calm	ESE.	...	...	...	...	64
13.	29·932	29·843	29·815	29·30	29·80	29·79	68·3	78·5	61·0	79	59	66·5	80	58	E.	E.	S.	E.	...	...	...	...	64
14.	29·892	29·803	29·589	29·29	29·78	29·75	66·8	75·3	62·0	78	55	65	60	59½	ENE.	SE.	calm	SE.	...	...	...	1·02	65
● 15.	29·966	29·886	29·847	29·28	29·68	29·62	67·7	71·4	62·6	66	55	63	66	57	S.	S.	calm	SE.	...	·06	...	...	65
16.	29·910	29·876	29·820	29·27	29·63	29·70	65·2	73·0	60·4	71	41	62	65	56½	S.	S.	calm	SW.	·063	·01	...	...	62
17.	30·036	29·967	29·913	29·40	29·80	29·85	58·3	69·6	50·6	69	41	57	65	52	W.	W.	calm	S.	...	...	·15	...	57
18.	29·964	29·903	29·892	29·43	29·86	29·88	54·7	67·7	49·8	72	40	56	64	42	S.	S.	calm	E.	...	...	...	...	56
19.	30·070	30·037	29·972	29·52	29·95	30·00	60·5	66·2	50·7	74	54	58	66	43	ENE.	E.	calm	E.	...	...	...	...	54
20.	30·184	30·109	30·056	29·60	30·09	30·10	62·5	68·7	60·0	71	60	62	65½	50½	E.	SE.	E.	E.	...	...	...	...	61
21.	30·018	29·969	29·700	29·54	30·06	29·90	60·3	70·0	59·7	70	56	59	59½	50	ENE.	E.	E.	ESE.	...	·10	...	0·05	61
☽ 22.	29·692	29·627	29·613	29·20	29·70	29·55	59·4	67·2	58·2	70	52	58·5	59	49	SE.	S.	E.	E.	·087	·30	...	...	61
23.	29·688	29·617	29·578	29·14	29·47	29·47	55·7	66·4	55·4	66	49	56	62½	50	S.	S.	calm	E.	·602	·23	·44	...	59
24.	29·578	29·526	29·480	29·05	29·43	29·40	58·3	62·2	56·0	64	50	57	57½	52	SSE.	SW.	calm	NE.	·366	·25	·04	...	53
25.	29·468	29·481	29·398	28·92	29·29	29·26	57·5	63·3	53·6	65	50	57	60	48	S.	SW.	N.	ENE.	·158	·30	·19	...	57
26.	29·504	29·480	29·422	28·94	29·29	29·30	58·2	63·6	53·7	64	50	54	60	48	SE.	S.	calm	E.	·080	·27	·03	...	56
27.	29·548	29·567	29·471	28·97	29·40	29·30	56·8	62·4	54·0	67	54	55	62	39	W.	W.	calm	ESE.	·327	·57	·02	...	57
28.	29·404	29·381	29·310	28·75	29·00	28·94	58·2	64·7	56·5	64	56	59	61½	51½	S.	S.	S.	S.	·438	·15	·39	1·95	58
29.	29·252	29·267	29·167	28·52	28·70	28·90	61·2	66·8	58·0	65	54	59	58	47	S.	S.	W.	SSW.	·088	·35	...	...	59
○ 30.	29·248	29·413	29·172	28·63	29·00	29·10	58·7	64·4	56·0	65	44	57	61	51	S var.	SW.	calm	SW.	·366	·11	·20	...	58
Mean.	29·804	29·746	29·670	29·20	29·594	29·594	60·1	67·3	51·7	68·83	49·66	58·3	62·2	45·3					Sum. 3·715	3·71	3·43	3·39	Mean. 58

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

DECEMBER 1841.

LXII. *First Sketch of some of the principal Results of a Second Geological Survey of Russia. Communicated by* RODERICK IMPEY MURCHISON, *Esq., F.R.S., President of the Geological Society.*

To the Editor of the Philosophical Magazine.

DEAR SIR,

IT was my earnest wish to have complied earlier with your request when I left this country, to send you from the spot some account of my distant wanderings; but the desire to avoid communicating early conceptions which might be modified by subsequent observation, induced me to stay my pen until I could offer something worthy of a place in the Philosophical Magazine. The short sketch which follows was written at Moscow near the close of the journey, and is, with some very slight alterations, the translation of a letter addressed to M. Fischer de Waldheim, the venerable and respected President of the Society of Naturalists of that metropolis. Since then, besides the official report to the Minister of Finance, the Count de Cancrine, I have submitted to His Imperial Majesty, a tabular view of all the formations in Russia, accompanied by a general map and a section from the Sea of Azof to St. Petersburgh. These documents, which will be engraved in the course of the winter, are to be considered only as the prelude to a long memoir with full illustrations of the organic remains, mineral structure and physical features of the country, which will be laid before the Geological Society of London, as soon as, with the assistance of my fellow-labourers, I shall have prepared the materials for the public eye. In the mean time the friends of science must be happy to learn, that

the Emperor, his ministers and officers have powerfully and kindly contributed to these results by every possible aid and support which geologists could receive. Desirous that this inquiry should be rendered as perfect as circumstances will admit, His Imperial Majesty has graciously authorized the Minister of Finance, the Count de Cancrine, to permit Count Keyserling to visit this country during the winter, to cooperate with myself, whilst General Tcheffkine, the chief of the Staff of the Mining Corps, and so well known to many of my English friends, has obtained permission for Lieut. Koksharoff to be among us for a season, to complete his studies, and acquire a correct knowledge of those *British* strata with which the deposits of our ancient allies and kind friends have been compared.

I remain, dear Sir, yours most faithfully,
RODERICK IMPEY MURCHISON.

16 Belgrave Square,
Nov. 5, 1841.

Letter to M. Fischer de Waldheim, Ex-President of the Society of Naturalists of Moscow.

(*Translation.*)

MY DEAR SIR, Moscow, Oct. 8, 1841.

As you have taken a lively interest in the success of the geological expedition which I have just completed, accompanied by my friends M. de Verneuil, Count de Keyserling, and Lieutenant Koksharoff, I hasten to communicate to you some of its chief results; and I do so with real pleasure, because in requesting you to present them to the Society of Naturalists of Moscow, I acquit myself of a duty towards a distinguished body which has done me the honour of placing my name in the list of its foreign members.

The wide extension in the North of Russia of the Silurian, Devonian and Carboniferous Systems, as proceeding from the last year's survey, by the same observers and our friend the Baron A. de Meyendorf, is already known to you from the abstracts of memoirs communicated to the Geological Societies of London and Paris. Our principal objects this year were,—1st. To study the order of superposition, the relations and geographical distribution of the other and superior sedimentary rocks in the central and southern parts of the empire. 2nd. To examine the Ural Mountains, and to observe the manner in which that chain rises from beneath the horizontal formations of Russia. 3rd. To explore the carboniferous region of the Donetz, and the adjacent rocks on the Sea of Azof.

Our last year's survey had pretty nearly determined the limits of the great tract of carboniferous limestone of the North of Russia. On this occasion we have added to its upper part

that remarkable mass of rock which forms the peninsula of the Volga near Samara, and which, clearly exposed in lofty, vertical cliffs, and charged with myriads of the curious fossils *Fusilina*, constitutes one of the striking features of Russian geology.

The carboniferous system is surmounted, to the east of the Volga, by a vast series of beds of marls, schists, limestones, sandstones and conglomerates, to which I propose to give the name of "Permian System," because, although this series represents as a whole, the lower new red sandstone (*Rohte todte liegende*) and the magnesian limestone or *Zechstein*, yet it cannot be classed exactly (whether by the succession of the strata or their contents) with either of the German or British subdivisions of this age. Moreover the British lithological term of lower new red sandstone*, is as inapplicable to the great masses of marls, white and yellow limestones, and gray copper grits, as the name of old *red* sandstone was found to be in reference to the schistose black rocks of Devonshire.

To this "Permian System" we refer the chief deposits of gypsum of Arzamas, of Kazan, and of the rivers Piana, Kama and Oufa, and of the environs of Orenbourg; we also place in it the saline sources of Solikamsk and Sergiefsk, and the rock salt of Iletsk and other localities in the government of Orenbourg, as well as all the copper mines and the large accumulations of plants and petrified wood, of which you have given a list in the 'Bulletin' of your Society (anno 1840). Of the fossils of this system, some undescribed species of *Producti* might seem to connect the Permian with the carboniferous æra; and other shells, together with fishes and Saurians, link it on more closely to the period of the Zechstein, whilst its peculiar plants appear to constitute a Flora of a type intermediate between the epochs of the new red sandstone or "trias" and the coal-measures. Hence it is that I have ventured to consider this series as worthy of being regarded as a "System."

The overlying red deposits which occupy a great basin in the governments of Vologda and Nijni Novogorod, have not as yet been found to contain any organic remains except minute *Cyprides* and badly preserved *Modiolæ;* but when we take into consideration their thickness, geological position, and mineral characters, we are disposed to think that they may at some future day be identified with a portion of the "Trias" of German geologists. I am strengthened in this opinion by Count Keyserling's discovering, during our tour at Monte Bogdo, certain fossils which are unknown in other parts of Russia, but which are associated with the *Ammonites Bogdoanus* already described by Von Buch, and which that distinguished geologist refers to the type of the muschelkalk.

* See Silurian System p. 54.

True lias does not exist in Russia, as Von Buch had decided from an examination of fossils sent to him, but the Jurassic or oolitic series is divisible into two stages. The lowest of these, which is much more developed than the upper, never occupies any considerable tract of country, being either distributed in patches, or hidden by newer accumulations. From the eastern flanks of the Ural chain in the 64° of N. latitude to the Caspian Sea, it preserves nearly the same mineral and fossil characters. This formation represents the inferior and middle oolite. The ferruginous sands, calcareous grits, and black schists of the Moskwa are of this age; and also those beds which we examined last year on the Volga between Kostroma and Kinshma, at Makarief upon the Unja, as well as those shales and sands which we have seen this year in many other localities, particularly between Arzamas and Simbirsk, between Syzran and Saràtoft, at Saragula, and on the river Ilek near Orenbourg.

The upper oolitic group occurs in several situations along the Donetz, where it was first recognized by Major Blöde. It is calcareous, often oolitic, of light yellow colour, and contains many *Trigoniæ*, *Nerineæ*, &c., which enable us to compare it with the upper Jura of the Germans, or Portland and Coral rag division of my own country.

The cretaceous system, though composed of very different beds of marls, white chalk, sands and grits (sometimes green), offers for the most part the fossils of the white chalk of Europe, such as the *Inocerami* (Catillus), *Belemnites mucronatus*, *Ostræa vesicularis*, *Terebratula carnea**.

Above the cretaceous system, we have not been able to discover in any part of Russia, except in the Crimæa, the "nummulite limestone" which there sets on, and acquires a great importance in its range through Georgia, Egypt, and the Mediterranean basin.

The equivalents of the lower tertiary formations (Eocene of Lyell) seem to exist in one part only of your country (S. of Saràtoft). On the other hand, the middle and upper tertiaries (Miocene and Pleiocene) cover large surfaces on the Lower Volga, in Podolia, Volhynia, and also along the shores of the Sea of Azof and the Black Sea, where the youngest of these strata, very much resembling the "upper crag" of Norfolk, are beautifully displayed.

I have not time to enter upon the numerous and inter-

* After this letter was written, we found in the collection of Professor Eichwald at St. Petersburgh, a fine specimen of Exogyra and other fossils in a green sandstone from the Lower Volga, sent to him from a locality well known to us, which leaves little doubt of the existence also of a true representative of our greensand.—R. I. M.

esting phænomena of the Ural Mountains, the examination of which occupied us nearly three months. We there studied alternately the wonders of the gold alluvia, the sites of the entombment of your great mammalia, and sought for the causes of the astonishing metamorphism of the sedimentary rocks of that chain. For an explanation of the last class of phænomena, the works of Humboldt and Gustaf Rose must always be consulted. I will on this occasion simply say, that far from being *primitive*, as was supposed, this chain, with the exception of its eruptive masses, is entirely composed of *Silurian*, *Devonian* and *Carboniferous* rocks, more or less altered and crystallized, but in which nevertheless we have been able to recognize in a great number of localities my own *Pentamerus Knightii**, and many fossils which clearly define the age of the other strata. These rocks, though much broken up, are arranged in parallel bands, the mean direction of which in the North Ural is from N. and by W. to S. and by E., whilst in the South Ural, trending N. and S., they assume a fan-shaped arrangement, spreading out towards the southern steppe of the Kirghis, where, interlaced with porphyries and other trap-rocks, they are often converted into the far-famed jaspers of this region.

Still less can I now pretend to treat of the great carboniferous region of the Donetz; for without entering into details concerning this southern tract, so valuable to the future interests of Russia, I cannot render it the justice which it merits. Still I may say to you as a geologist, that its numerous beds of coal (bituminous and anthracitic), with its grits and shales, are completely subordinate to the mountain limestone series, and represent in no sense the coal-fields of Great Britain, Belgium, and France.

In concluding, however, I must tell you of a very interesting discovery we made in returning from Taganrog to Petersburgh. Count Keyserling took the line of Voroneje and the Don, and M. de Verneuil and myself that of Koursk, Orel and the river Oka, and on meeting at Moscow our results completely agreed.† It was, as you know, generally believed up to this moment, that central Russia presented a regular succession from older to younger deposits as you proceeded from north to south. This is not the case. A great axis of Devonian rocks or old red sandstone, having a width of at least 120 miles, rises in the heart of the country around Voroneje and Orel, and stretches to the W. N. W., in which

* Silurian System, p. 615.

† Colonel Helmersen, so distinguished for his geographical and geological researches in Russia, also examined the tract near Orel in the course of the summer, and had come to the same conclusions as our party. I was however unacquainted with his opinion when I wrote this letter.—R. I. M.

direction it probably connects itself with deposits of the same age in Lithuania and in Courland. This discovery seems, indeed, to have an intimate relation to one which we made in entering Russia early in the spring, near to Schavli in Lithuania, of much red ground and a band of upper Silurian rocks. In fact it also explains the cause of the great difference which exists between the deposits of the carboniferous basin of the Donetz and those of your Moscow region, now proved to constitute a vast *basin*. For as the two seas, in which these deposits were accumulated from high antiquity, were separated by the ancient lands in question, so must we infer that the conditions and nature of their shores, their rivers, their currents and bottoms (on which of course the nature of marine deposits depend), must have been essentially different.

This discovery also proves the symmetry of the opposite edges of the *Moscow basin;* since in advancing from the governments of Tula and Kaluga on the south, we see the same ascending order as that which we before described in the Waldai Hills on the north. In both tracts the Devonian or old red rocks, with *Holoptychius Nobilissimus*, and many fishes and shells of that system well known in the British Isles*, pass under the lowest strata of the carboniferous æra, and serve as a base line to those thin beds of poor coal associated with *Unio sulcatus* and *Productus gigas* (*hemisphericus*, Sow.), which are at present the subject of new researches on the part of the Russian Government.

The enormous space we traversed and examined, in all between 13 and 14 thousand miles, might well astonish you, if I did not assure you, that the arrangements for this journey, undertaken under the auspices of the Minister of Finance, Count de Cancrine, were admirably prepared by General Tcheffkine, whose clear directions, united to that spirit of hospitality which characterizes all Russians, and above all the inhabitants of the Ural and Siberia, rendered every enterprise feasible, and enabled us to overcome every obstacle.

I shall communicate to you at a later date, and before our large memoir is prepared, the general table of the order of superposition of all the formations of Russia, with sections†.

Accept, dear Sir, the assurance of the affection and esteem of your devoted servant,

RODERICK IMPEY MURCHISON,
President of the Geol. Society of London.

To His Excellency M. Fischer de Waldheim.

* See Silurian System, p. 599.

† These documents, which were laid before His Imperial Majesty in MSS., are now in the hands of the engraver.

LXIII. *On the Theory of Storms, with reference to the Views of* Mr. Redfield. *By* Robert Hare, *M.D., Member of the American Philosophical Society, Professor of Chemistry in the University of Pennsylvania**.

1. MR. REDFIELD'S idea, that tornadoes and hurricanes are all whirlwinds, involves some improbabilities.

2. It requires that during every hurricane there should be blasts of nearly equal force coinciding with every tangent which can be applied to a circle. Thirty-two ships, equidistant from the axis of gyration and from each other, should *each* have the wind from a different point of the compass with nearly equal force. The only modification of which this view of the case admits, is that resulting from the progressive motion which tends to increase the velocity of the wind on the side on which this motion concurs with that of the whirl, and to produce upon the opposite side a corresponding diminution. Moreover, as respects any one station, the chance would be extremely unfavourable that the same hurricane should twice proceed from the same quarter: and yet in the course of time it would be felt, at any station, to proceed from many different directions, if not from every point of the compass.

3. Mr. Redfield has alleged, that he observed proofs of gyration in the effects of the New Brunswick tornado; but I think that the survey of Bache and Espy shows that it would not be consistent with the facts to suppose such a motion, unless contingently; and that it could only be a casual effect of the currents rushing towards the axis of the tornado†.

4. Being of opinion that calorific expansion is inadequate to explain the afflux of wind towards the equator, it is alleged by Mr. Redfield, "that the space previously occupied by the atmosphere so left behind, is, by the centrifugal action of the earth's rotation, constantly supplied from higher latitudes."

5. I presume that the meaning of this allegation is, that the centrifugal force communicated to the air at the equator by the diurnal revolution of the earth, lessening the gravity of the air thus affected, causes it to rise, and give place to those portions of the atmosphere which, existing where the diameter of the earth is less, have less rotary motion. Admitting an afflux to arise in this way, could it have any other effect than that of accumulating air over the equator, compensating by quantity and altitude for the loss of weight arising from a greater centrifugal force pertaining to that region? But, on

* Communicated by the Author.

† See fifth volume of the American Philosophical Transactions.

the other hand, if we attribute the ascent of the air at the equator to heat, the theory of calorific circulation will account for the continuance of the process.

6. In ascribing the prevalence of westerly winds in the upper regions of the atmosphere to the deflection of the trade winds by our mountains, Mr. Redfield's explanation harmonizes with the theory of Halley. In fact, is it not reasonable, that, as the water accumulated by these winds in the Gulf of Mexico is productive of a gulf-stream, there should be an *aërial* accumulation and current corresponding with that of the aqueous current, which is designated by the name above-mentioned?

7. But not perceiving that the trade winds cannot be explained without the agency of temperature, Mr. Redfield, in the following paragraph, rejects the influence of heat:—

8. " To me it appears that the causes of the great storms may be considered to indicate with entire certainty the great law of circulation in our atmosphere; and that the long-cherished theory, which is founded on calorific rarefaction, must give place to a more natural system of winds and storms, founded mainly upon the more simple conditions of the great laws of gravitation."

9. It would seem from this paragraph as well as others, that Mr. Redfield admits of no other cause of atmospheric currents besides that of gravitation. But in the absence of calorific and electrical reaction, what other effect could gravitation have, unless that of producing a perfect state of inert quiescence?

10. It is remarkable that the author, after ascribing the trade winds to momentum, as the antagonist of gravitation, loses sight of it in this summing up of the causes of atmospheric currents!

11. If, as Mr. Redfield alleges, the minuteness of the altitude of the atmosphere, when compared with its horizontal extent, be an objection to any available currents being induced by calorific rarefaction, wherefore, for the same reason, should not momentum, or any other cause diminishing or counteracting the influence of his chosen agent, gravity, be equally inefficient?

12. Assuming that the motion of the air in hurricanes is always gyratory, Mr. Redfield considers gyration as a *cause* of these terrible meteors! How far his language on this subject is reasonable or consistent, may be seen from the following paragraph, which I quote from one of his essays, published in Silliman's Journal for 1834, vol. xxv. page 125:—

13. " Notwithstanding these general and determinate ho-

rizontal movements, the equal distribution of the atmosphere over the snrface of the globe,which results from gravitation, tends to prevent any very rapid or violent motion in any specific direction, and consequently to prevent violent and destructive winds. But owing to the tendency of all fluid matter to run into whirls or circuits, when subject to the influence of unequal or opposing forces, a rotative movement of unmeasured violence is sometimes produced. This peculiar movement, which in its most active state is sometimes distinguished by the name of tornado or hurricane, assumes every possible variety of position, appearance, velocity and extent, and is the only known cause of violent and destructive winds or tempests."

14. Agreeably to this paragraph, gravitation, in lieu of being, as previously alleged, the main basis of winds and storms, tends to produce that equal distribution of the atmosphere over the surface of the globe, on which I have insisted.

15. But if neither gravity, nor calorific expansion, nor electricity be the cause of winds, by what *are* they produced?

16. He alleges that the fluid matter has a tendency to run into whirls or circuits, when subject to the influence of unequal or opposing forces, and that in this way a rotative movement of unmeasured violence is sometimes produced.

17. If this were true, evidently whirlpools, or vortices of some kind, ought to be as frequent in the ocean, as, agreeably to his observation, they are found to be in the atmosphere. The aquatic gulf-stream, resulting from the impetus of the trade winds, ought to produce as many vortices in its course as the aërial currents derived from the same source, especially as in the ocean the great laws of gravitation have full liberty to act without any important interference from calorific changes, to which the advocates of the agency of such changes in producing wind will not ascribe much efficacy, where non-elastic liquids are in question.

18. There are few vortices or whirlpools in the ocean, because there are in few cases descending currents towards which the surrounding waters are concentrated. Of course, vertical currents cannot arise from any imaginable cause.

19. The conflicts of "*opposing or unequal forces*" do not produce curvilinear motion, unless there be a successive deflection, as in the case where it results from centripetal force, or the influence of gravity upon a projectile. If one of two *directly opposite* forces be less than the other, retardation will ensue, and a lateral current or currents carrying off the excess of momentum. If currents encounter each other *obliquely*, a diagonal current will result. I doubt if a whirlpool ever

takes place without a centripetal force resulting from an hiatus.

20. But the author has not informed us how these unequal or opposing forces are generated in the atmosphere. Without any assigned cause, he appeals to "certain opposite and unequal forces by which a rotative movement of unmeasured violence is produced." This rotative movement, although alleged, as above, to be an *effect*, is stated immediately afterwards to be "the only known cause of violent and destructive winds or tempests."

21. In a memoir on the causes of tornadoes and water-spouts, and in some subsequent communications published in the Transactions of the American Philosophical Society, and republished in Silliman's Journal, various facts and arguments were mentioned, tending to prove that the proximate cause of the phænomena of a tornado is an ascending current of air, and the afflux of wind from all points of the compass to supply the deficiency thus created.

22. In this mode of viewing the phænomena, no difference of opinion exists between Bache and Espy and myself, however we may differ respecting the cause of the diminution of atmospheric pressure within the track of a tornado, which gives rise to the ascending current.

23. I adduced several facts, upon the authority of the skilful survey made by those gentlemen, proving that the effects were in some cases inconsistent with the existence of a *whirl*; and I mentioned one which could not be explained without attributing it to a gyratory force. I was led to consider gyration as a contingent, not an essential feature in the meteors in question.

24. It appeared reasonable to suppose that the confliction of confluent streams of air rushing towards an axis moving progressively, might be productive of a whirling motion. The contortion of six feet of the upper part of a brick chimney upon the lower portion, so as to cause the corners of either portion to project over the sides of the other, was deemed inexplicable without ascribing it to gyratory force. Subsequently, however, it occurred to me that this fact was more likely to be the result of a *local* than of a *general* whirl; since in the latter case, the chimney could not have been twisted as described, without being precisely at the centre of the whirlwind. That such could have been its position, appears to me extremely improbable; and had it been so situated, as the whirlwind was estimated to be moving progressively at the rate of seventeen miles per hour, it is to me incomprehensible how the portion which was dislocated could have escaped an overthrow. Evidently, although twisted upon its base while

concentric with the gyration, it would in one second of time have been twenty feet upon the windward side of it, consequently subject to the tangential force of the whirlwind. I adduced this, as well as other facts, to prove that in tornadoes and hurricanes there are local whirls, causing bodies which are of a nature to favour electrical discharge to be particularly affected; a fact which is admitted by Mr. Redfield, was considered by Espy and Bache, as well as myself, to be irreconcilable with the idea that a general whirling motion is essential to tornadoes. I allude to the circumstance, that when several trees were prostrated one upon the other, the uppermost was found to have fallen with the top directed towards the point towards which the meteor was moving; while the direction in which the lowermost trees were found to have fallen indicated that they were overthrown by a force in a direction precisely the opposite of that which had operated upon those above-mentioned.

25. Mr. Redfield has not made any effort to show how the trees could have been piled upon each other, as described, but, on the contrary, admits that a whirlwind would blow oppositely, on opposite margins of the whirl. As this appears to me quite evident, I cannot understand how the opposite forces belonging respectively to the different sides of the whirlwind, can be made to bear successively upon one spot, so as to cause trees to fall in diametrically opposite directions.

26. Another fact, irreconcilable with a general whirling motion, was adduced by Messrs. Espy and Bache. One of the four posts, upon which a frame building was supported, was first moved towards the tornado, as it advanced; in the next place as it moved away, so as to make two furrows in the ground. In the interim the frame was protected by a larger building, which intervened between it and the tornado. I am utterly unable to understand how the transient tangential forces of a whirlwind blowing oppositely, on the opposite margins of its track, could thus move the post in question, so as to make two distinct furrows in the ground indicating two successive impulses, in directions of which one was at right angles with the other. Mr. Redfield admits that "the confused directions of fallen bodies is distinctly recognized by all the parties to this inquiry." Conceding that amid this confusion he has been enabled, by a survey, to show that the directions in which certain trees fell are consistent with their having been subjected to a whirlwind, it does not demonstrate gyration to be an essential feature of tornadoes. It is sufficiently accounted for by considering it as a fortuitous consequence of the conflict of currents rushing into a rarefied vortex.

27. Mr. Redfield adopts the singular determination of not noticing the "insuperable difficulties" of the hypothesis which he has undertaken to set aside. The advocates of the disputed hypothesis are not aware of any such difficulties: is it correct to allege their existence without mentioning the facts and arguments which justify this allegation? Without repeating here the evidence and the reasoning which I have already published on this subject, I will advert to one fact which is utterly irreconcilable with Mr. Redfield's "rotary theory." I allude to the statement of a most respectable witness, that while the tornado at Providence was crossing the river, the water, which had risen up as if boiling within a circle of about 300 feet, subsided as often as a flash of lightning took place. Now supposing the water to have risen by a deficit of pressure resulting from the centrifugal force of a whirl, how could an electrical discharge cause it to subside?

28. I have already, I trust, sufficiently shown that the abortive explanation which Mr. Redfield dignifies with the title of "his theory of rotary storms," amounts to no more than this: that certain imaginary, nondescript, unequal and opposing forces produce atmospheric gyration; that these gyrations, by their consequent centrifugal force, create about the axis of motion a deficit of pressure; and hence the upward force displayed by tornadoes and hurricanes. I cannot give to this alleged theory the smallest importance, while the unequal and opposing forces upon which it is built remain in perfect obscurity, the author having disclaimed both the agency of heat and electricity.

29. But admitting a whirlwind to be produced, not by a deficit of pressure about the axis, but by unequal and opposing forces acting externally in any competent way whatever, it is perfectly evident that any deficit of pressure about the axis consequent to the resulting centrifugal force, could only cause a descending aërial current, while it could not tend in the slightest degree to carry solids or liquids aloft.

30. It must be obvious that the stratum of air on the earth's surface, partaking of the circular motion, must also partake of the centrifugal momentum, and of course would have a disposition the very inverse of that which would cause them to rush towards the axis; while heavier bodies being surrounded by the rarer mediums consequent to the whirl, would have their gravity less counteracted than usual. I cannot help thinking, that as respects the application of his "rotary theory" to explain tornadoes, these arguments will amount to a "*reductio ad absurdum.*"

31. Mr. Redfield infers that the whirlwinds, of which he as-

sumes the existence, have a property which he alleges to be observable in "all narrow and violent vortices, viz. a spirally involute motion quickened in its gyrations as it approaches towards the centre of the axis or whirl."

32. But is it not evident, that if any mass of matter be made to revolve by unequal and opposing forces, or by any other than those resulting from the centripetal force, caused, as already described, by an ascending current, the gyration will not quicken in proportion as the gyrating matter may be nearer the centre, but on the contrary will be slower as the distance from the axis may be less? It appears to me, that the only case in which gyration is found to quicken in proportion as the matter involved approaches the vortex, is that which results from the confluence caused by an ascending or descending concentric current. So far therefore as Mr. Redfield's observations confirm the idea that the whirling motion in tornadoes quickens towards the centre, it tends to confirm the opinions which he combats, and to refute those which he upholds.

33. To conclude whether or not the efforts which I have made, to show that the phænomena of tornadoes and hurricanes arise from an electrical discharge by convection, be justifiable, I think it will be conceded that any theory of storms which overlooks the part performed by electricity must be extremely defective.

34. Both by Messrs. Espy and Redfield the influence of this agent in the phænomena of nature is entirely disregarded, although with the storms, which have been especially the subject of their lucubrations, thunder, lightning, and convective discharge are most strikingly associated.

35. I will conclude with subjoining the following propositions, inferences, or allegations; which are so evident to my mind, that I am at a loss to understand that they have not had a similar influence upon the minds of all who are conversant with the science of electricity.

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36. Our experiments make us familiar with two processes of electrical discharge. In one of these electricity passes in the form of sparks or flashes; in the other it may be conveyed without any perceptible evolution of light, by the alternate or successive contact of intervening bodies with the excited surfaces, as, for instance, by means of pith-balls, pendula, or a blast of air. The former process has been designated by Faraday as disruptive, the latter as convective discharge.

37. The disruptive process being exemplified by lightning,

the magnificent apparatus of nature by means of which this awful phænomenon is displayed, may be supposed competent to produce convective discharge upon a scale of proportionable magnitude, as exhibited in tornadoes and hurricanes.

38. As bodies oppositely electrified attract each other, *à fortiori,* attraction must always exist between any bodies sufficiently electrified for an electric discharge to take place between them. This law may be illustrated by means of an instrument called Cuthbertson's electrometer. Hence the rising of water within the track of a tornado, and its subsidence on the passage of lightning, as observed by Mr. Allen near the city of Providence, Rhode Island, may be considered as resulting from the alteration of convective with disruptive discharge*.

39. By this observation of Mr. Allen, attraction is shown to have existed between an electrified stratum of air coated by clouds, and the oppositely electrified water of a subjacent river. It is reasonable to infer, that attraction, originating in the same way, operating upon the denser stratum of the atmosphere in the vicinity of the earth by counteracting gravitation, may cause that rarefaction by which houses are burst or unroofed,

* "The most interesting appearance was exhibited when the tornado left the shore, and struck the surface of the adjacent river. Being within a few yards of this spot, I had an opportunity of accurately noting the effects produced on the surface of the water.

"The circle formed by the tornado on the foaming water was about three hundred feet in diameter. Within this circle the water appeared to be in commotion, like that in a huge boiling caldron; and misty vapours, resembling steam, rapidly arose from the surface, and entering the whirling vortex, at times veiled from sight the centre of the circle, and the lower extremity of the overhanging cone of dark vapour. Amid all the agitation of the water and the air about it, this cone continued unbroken, although it swerved and swung around, with a movement resembling that of the trunk of an elephant whilst that animal is in the act of depressing it to the ground to pick up some minute object. In truth, the tapering form, as well as the vibrating movements of the extremity of this cone of vapour, bore a striking resemblance to those of the trunk of that great animal.

"Whilst passing off over the water, a distant view of the cloud might have induced the spectator to compare its form to that of a huge umbrella suspended in the heavens, with the column of vapour representing the handle, descending and dipping into the foam of the billows. The waves heaved and swelled whenever the point of this cone passed over them, apparently as if some magical spell were acting upon them by the effect of enchantment. Twice I noticed a gleam of lightning, or of electric fluid, to dart through the column of vapour, which served as a conductor for it to ascend from the water to the cloud. After the flash the foam of the water seemed immediately to diminish for a moment, as if the discharge of the electric fluid had served to calm the excitement on its agitated surface." — See Transactions of the American Philosophical Society, vol. vi.

and an upward current of tremendous force produced. We may also infer that bodies are carried aloft by the joint action of electrical attraction, and the vertical blast which it produces.

40. The effects upon the leaves of trees noticed by me after the tornado of New Brunswick, in 1835, and still more those subsequently observed by Peltier after that of Obatenage in 1839, cannot be explained without supposing them to have been the medium of an electric discharge *.

41. When a convective discharge takes place between a stratum of air in proximity to the earth, and a stratum in the region of the clouds, the greater density and pressure of the lower stratum will determine the current to rush up in a vertical direction.

42. Experience has demonstrated that electricity cannot exist on one side of an electric, without its existence simultaneously upon the other side. If the interior of a hollow globular electric be neutral, so will the outside be; but if the interior be either positively or negatively excited, the outside will be found in the one case negative, in the other positive.

43. The atmosphere consists of an electric in the hollow globular form; and as electricity is known to pervade the space within it occupied by earth, the principle in question must also pervade the space beyond that portion of the atmosphere which is sufficiently dense to insulate or to perform the part of an electric.

44. Thus there are three enormous concentric spaces, of which the intermediate one is occupied by an electric, while the innermost one and the outer one are occupied by conductors. The two last-mentioned may be considered as equivalent to two oceans of electricity, of which one may be called the celestial, the other the terrestrial electric ocean.

* "In this hasty account I have, with the intention of returning to this portion of the subject, omitted to speak particularly of its effect upon trees. All those which came within the influence of the tornado, presented the same aspect; their sap was vaporized, and their ligneous fibres had become as dry as if kept for forty-eight hours in a furnace heated to ninety degrees above the boiling point. Evidently there was a great mass of vapour instantaneously formed, which could only make its escape by bursting the tree in every direction; and as wood has less cohesion in a horizontal longitudinal than in a transverse direction, these trees were all, throughout one portion of their trunk, cloven into laths. Many trees attest, by their condition, that they served as conductors to continual discharges of electricity, and that the high temperature produced by this passage of the electric fluid, instantly vaporized all the moisture which they contained, and that this instantaneous vaporization burst all the trees open in the direction of their length, until the wood, dried up and split, had become unable to resist the force of the wind which accompanied the tornado."

45. When either electric ocean is minus, the other must be plus, and at the same time any intermediate stratum of the atmosphere inclosing a stratum of clouds must be charged by induction.

46. Between the concentric strata of air, severally bounding the celestial and terrestrial ocean, there must be an electrical attraction tending to counteract gravitation, and thus to influence the density and pressure of the lower stratum of the atmosphere.

47. The proximities of a stratum of clouds electrified by the celestial ocean, must cause an accumulation of electricity in any portion of the terrestrial surface immediately subjacent; and by counteracting gravitation cause a local diminution of atmospheric pressure, which it is well known is a precursor, and probably a cause of wind and rain.

48. Those common discharges of electricity which take place during hurricanes, may be easily accounted for by supposing that they result from discharges between the celestial and terrestrial electric oceans.

49. Thunder-clouds may owe their charges to the celestial ocean, either by induction or conduction. Auroras may be the consequence of discharges from one part of the atmosphere to another, through the rare conducting medium which is occupied by the celestial ocean; or they may result from discharges from other planets or seas, or from any part of space, however remote. Since, agreeably to Wheatstone's experiments, electricity flies with a velocity not less than that of light, space can create no obstacle to its passage.

LXIV. *On the Rotation of a rigid Body round a fixed Point.*

By James Booth, *Esq., M.A., Principal of, and Professor of Mathematics in Bristol College**.

I. THE problem of the rotation of a rigid body round a fixed point, acted on by no forces, or round its centre of gravity influenced by the force of gravity alone, has been analytically solved by Lagrange, at least so far as to indicate the leading properties of such motion, and to reduce its determination to the calculation of two elliptic functions, one of the first, the other of the third order; but these analytical formulæ, as has been justly remarked, do not give us any clear idea of the motion during the period of rotation; they enable us to determine the position of the body at the end of a given time, but do not at all assist our conceptions in following the motion of the body during its rotation.

* Communicated by the Author.

Some years ago, however, this defect of the theory was removed in a memoir presented to the French Institute, by an author remarkable as well for the originality as the perspicuity of his views, in which he reduces the motion of the body to that of an ellipsoid whose centre is fixed, rolling on a fixed plane.

In the very brief extract which has been published of this memoir, Poinsot assumes an ellipsoid whose centre coincides with the fixed point, and whose semiaxes are proportional to the inverse square roots of the moments of inertia, round the three principal axes passing through the fixed point, coinciding with the axes of the ellipsoid; and thence by the known geometrical properties of this surface, and the tangent plane, deduces the nature of the motion, with great elegance and simplicity.

In the following pages an ellipsoid is assumed different from that of Poinsot, in which the squares of the axes are proportional to the moments of inertia round the principal axes of the body passing through the fixed point, coincident with the axes of this ellipsoid, which may be termed the *Ellipsoid of moments*.

It becomes proper to mention, that as the two ellipsoids of moments, that assumed by Poinsot, and the one here adopted, are *reciprocal surfaces*, all the properties of such motion may be indifferently deduced on either system, and then at once transferred to the other, by the known properties of reciprocal surfaces; a few examples of such transformation are given towards the close of this paper.

It appears, however, the more eligible course to deduce the general properties of rotatory motion, directly and independently, from the fundamental and acknowledged principles of mechanics, than to have recourse to the aid of reciprocal surfaces, with the relations of which many readers may not be familiar.

Previous to entering on the subject to which this paper is more particularly devoted, it may be well to state and prove a few general propositions bearing on the subject.

II. Let Ox, Oy, Oz, be three rectangular axes passing through the fixed point O, the axe Oz being the instantaneous axis of rotation of the body*, X, Y, Z the forces which act on any particle dm of the body, of which the coordinates are $x\,y\,z$; these forces being translated to the origin, are there equilibrated by the resistance of the fixed point O, while

* The existence of an axis of instantaneous rotation is assumed, as a proof of it from elementary principles may be easily given. (See Earnshaw's Statics, art. 109.)

they generate the moments $(Yx - Xy)\,dm$, $(Zy - Yz)\,dm$, $(Xz - Zx)\,dm$, in the planes of xy, zy, and xz respectively; hence the sum of the moments generated by the forces which act on the whole mass are $\int(Yx - Xy)\,dm$, $\int(Zy - Yz)\,dm$, and $\int(Xz - Zx)\,dm$, in the above-named planes.

III. Let the axis Oz be the instantaneous axis of rotation, ω the angular velocity round this axe, then the effective forces are, $X = -\omega y$, $Y = \omega x$, $Z = 0$; substituting these values in the above formulæ, we find the moments $\omega\int(x^2 + y^2)\,dm$ $-\omega\int xz\,dm$, and $-\omega\int yz\,dm$ in the planes of xy, yz, and xz respectively.

Now as the impressed and effective moments must by the principle of D'Alembert be equivalent, we shall have, denoting by K the impressed moment, and by $\alpha\,\beta\,\gamma$ the angles which its axis makes with the axes of coordinates, the following equations:—

$$\left.\begin{array}{c} K\cos\alpha = -\omega\int xz\,dm,\ K\cos\beta = -\omega\int yz\,dm, \\ K\cos\gamma = \omega\int(x^2 + y^2)\,dm \quad \ldots\ldots \end{array}\right\} \quad (1.)$$

From these equations it follows that when $\int xz\,dm = 0$, $\int yz\,dm = 0$, or when z is a principal axis of the body, that $\gamma = 0$, or that the plane of the impressed moment must be perpendicular to a principal axe, in order that this axe may be an axis of rotation; and the angular velocity round this axe is $= \dfrac{K}{\int(x^2+y^2)\,dm}$.

IV. The centrifugal forces generated by the motion act in the direction of the radius vector, and are proportional to the square of the velocity divided by the radius; hence $X = \omega^2 r \cdot \dfrac{x}{r}$ $= \omega^2 x$, $Y = \omega^2 y$, $Z = 0$; translating these forces to the origin, the moments thence resulting are $\omega^2\int xz\,dm$, $-\omega^2\int yz\,dm$ and 0, in the planes of xz, yz, and xy.

Let G be the centrifugal moment, α', β', γ' the angles which its axis makes with the axes of coordinates, then

$$\left.\begin{array}{c} G\cos\alpha' = -\omega^2\int yz\,dm, \quad G\cos\beta' = \omega^2\int xz\,dm, \\ G\cos\gamma' = 0. \quad \ldots\ldots\ldots\ldots \end{array}\right\} \quad (2.)$$

V. *The plane of the centrifugal moment* G *passes through the instantaneous axis of rotation, and through the axis of the impressed moment* K.

For as γ' is a right angle, the plane of G passes through the axis of z, the instantaneous axis of rotation.

Again, let V be the angle between the planes of G and K, then $\cos V = \cos\alpha\cos\alpha' + \cos\beta\cos\beta' + \cos\gamma\cos\gamma'$. Putting for

these angles their values given by (1.) and (2.), we find

$$K\,G\cos V = (\omega^3 - \omega^3)\,.\int y\,z\,d\,m\,.\int x\,z\,d\,m = 0,$$

hence $V = 90$, or the plane of G passes through the axis of K.

VI. To express G in terms of K, ω, and the moment of inertia round the instantaneous axis of rotation.

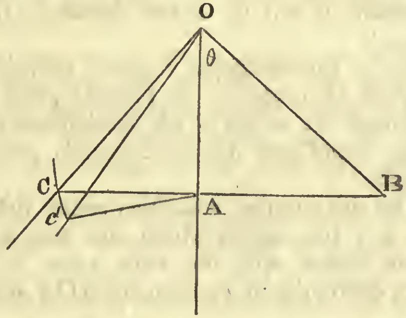
Squaring the six equations (1.) (2.), adding the first three together, and also the last three, we find

$$K^2 = \omega^2\,(\int x\,z\,d\,m)^2 + \omega^2\,(\int y\,z\,d\,m)^2 + \omega^2\,(\int (x^2+y^2)\,d\,m)^2$$
$$G^2 = \omega^4\,(\int x\,z\,d\,m)^2 + \omega^4\,(\int y\,z\,d\,m)^2.$$

Multiplying the first of these equations by ω^2, subtracting the second from the first, and calling the moment of inertia round the instantaneous axis of rotation D, we get

$$G^2 = K^2\,\omega^2 - \omega^4\,D^2 \quad \ldots\ldots\ldots \quad (3.)$$

VII. Let ω' denote the angular velocity round any line which makes the angle θ with the instantaneous axis of rotation, ω the angular velocity round this axis, then $\omega' = \omega\cos\theta$.



Let O A be the instantaneous axis, O B a line passing through the fixed point O, making the angle θ with the former O C, C A right lines perpendicular to O B, O A; let the point C move to c, a point indefinitely near the former, then $C\,c = O\,C\,.\,\omega'$, and $C\,c = C\,A\,.\,\omega$; hence $O\,C\,.\,\omega' = C\,A\,.\,\omega$, but $C\,A = O\,C\cos\theta$; hence $\omega' = \omega\cos\theta$.

VIII. Conceive an ellipsoid whose centre coincides with the origin, whose axes coincide with the principal axes of rotation of the body passing through the fixed point, and the squares of whose semiaxes are proportional to the moments of inertia round these axes, so that

$$A = n^3\,a^2, \quad B = n^3\,b^2, \quad C = n^3\,c^2 \quad \ldots \quad (4.)$$

n being a constant, a, b, c the semiaxes of the ellipsoid, and A B C the moments of inertia round those axes.

Let D be the moment of inertia round a right line passing through the centre, making the angles λ, μ, ν with the axes of coordinates, then

$$D = A\cos^2\lambda + B\cos^2\mu + C\cos^2\nu \quad . \; . \; . \; . \; . \quad (5.)$$

Poisson, *Traité de Mécanique*, tom. ii. p. 56.

Or, putting for A, B, C their values given by (4.),

$$D = n^3 (a^2\cos^2\lambda + b^2\cos^2\mu + c^2\cos^2\nu).$$

Now the part within the brackets is the square of a perpendicular from the centre of the ellipsoid on a tangent plane, making the angles λ, μ, ν with the axes, calling this perpendicular P, we have

$$D = n^3 P^2 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (6.)$$

IX. Assume the impressed moment

$$K = n^3 f u, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (7.)$$

u being the central semidiameter of the ellipsoid perpendicular to the plane of the impressed moment K. The product fu is of course constant, it will be shown presently that f and u are each constant.

X. *The axis of instantaneous rotation coincides with the perpendicular from the centre on the tangent plane to the ellipsoid, drawn through the vertex of the semidiameter u, and the angular velocity round this axis is inversely proportional to this perpendicular.*

Let p, q, r be the angular velocities round the three principal axes, α, β, γ the angles which the axis u of the impressed moment makes with the same axes, x, y, z the coordinates of the extremity of u, then by (III.) we have

$p = \frac{K\cos\alpha}{A}$, now $K = n^3 f u$, $\cos\alpha = \frac{x}{u}$, $A = n^3 a^2$, hence

$p = \frac{fx}{a^2}$; in like manner

$$q = \frac{fy}{b^2}, \quad r = \frac{fz}{c^2} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (8.)$$

$$\text{Hence } p^2 + q^2 + r^2 = \omega^2 = f^2 \left\{ \frac{x^2}{a^4} + \frac{y^2}{b^4} + \frac{z^2}{c^4} \right\} = \frac{f^2}{P^2} \quad (9.)$$

But the cosines which the axis of instantaneous rotation makes with the axes of coordinates, are $\frac{p}{\omega}, \frac{q}{\omega}, \frac{r}{\omega}$; and from the values of p, q, r just found, we get

$$\frac{p}{\omega} = \frac{Px}{a^2}, \; \frac{q}{\omega} = \frac{Py}{b^2}, \; \frac{r}{\omega} = \frac{Pz}{c^2}.$$

Now the cosines of the angles which the perpendicular to the tangent plane touching the surface at the point $(x y z)$ makes with the axes of coordinates, are also $\frac{\mathrm{P}x}{a^2}, \frac{\mathrm{P}y}{b^2}, \frac{\mathrm{P}z}{c^2}$; hence the axis of instantaneous rotation coincides with this perpendicular.

XI. *During the whole motion of rotation the semidiameter of the ellipsoid perpendicular to the plane of the impressed moment is invariable, or u is constant*; to show this we shall make use of the following property of the ellipsoid :—

Let a tangent plane be drawn to an ellipsoid, u the semidiameter through the point of contact, P the perpendicular on this tangent plane from the centre, m the semidiameter of the ellipsoid perpendicular to the plane of u and P; m and u are the semiaxes of the section of the surface made by the plane containing u and m.

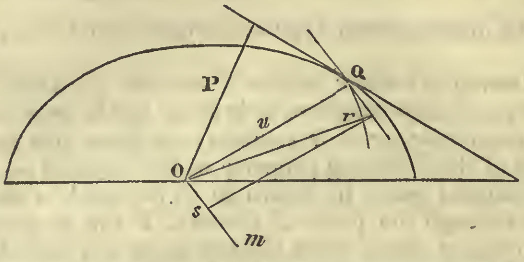
As P is perpendicular to the tangent plane, every plane which passes through this line is perpendicular to this plane, hence the plane containing u and P is perpendicular to the tangent plane.

In like manner, as the semidiameter m of the ellipsoid is perpendicular to the plane of u and P, the plane which passes through m and u, is perpendicular to the plane containing u and P; hence the tangent plane, and the plane of m and u, are each perpendicular to the plane of u and P, therefore their intersection is perpendicular to the same plane, and therefore parallel to m, and therefore perpendicular to u; but when a tangent to a conic section is perpendicular to the diameter passing through the point of contact, this diameter is an axe of the section; therefore u and m are the semiaxes of the section of the ellipsoid containing u and m.

Now u being the axis of the impressed moment, and P the perpendicular on the tangent plane, through the vertex of u, P is the axis of instantaneous rotation, therefore the plane of u and P is the plane of the centrifugal moment, and m perpendicular to this plane (V) is its axis.

Assume a point s, on this line m, so that $o\,s$ may be to u as G to K; and complete the rectangle O s Q v, the diagonal $o\,v$ of this rectangle will represent both in magnitude and direction, the axis of the resultant moment at the end of the first instant; during this instant the vertex of the axe of the resultant moment has travelled on the surface of the ellipsoid, and also on the surface of a concentric sphere, whose radius is u, since the line Q v perpendicular to O Q or u is a tangent both to this sphere and the ellipsoid; hence at the

end of the first instant the vertex of the axe of the resultant moment will be found on the curve of double curvature the intersection of the sphere and ellipsoid; therefore u is constant.



XII. Now as u is constant, and fu also constant (7.), f too is constant; but the angular velocity

$$\omega = \frac{f}{P}; \text{ (see (9.))}$$

hence the angular velocity round the instantaneous axis of rotation varies inversely as P.

XIII. *The angular velocity* ϖ *round the axis of the impressed moment is constant during the motion.*

Let θ be the angle between u and P, $\cos\theta = \frac{P}{u}$, and $\varpi = \omega\cos\theta$ hence

$$\varpi = \frac{f}{P}\cdot\frac{P}{u} = \frac{f}{u}, \text{ a constant quantity} \quad . \; . \; . \quad (10.)$$

XIV. *The magnitude of the centrifugal moment* G *varies as the tangent of the angle between u and* P.

Resuming the equation (3.), $G^2 = K\,\omega^2 - \omega^4 D^2$, and putting for K, ω, and D their values given by (7.), (9.) and (6.) and introducing the relation $P^2 \tan^2\theta = u^2 - P^2$, we find

$$G = K\frac{f}{u}\tan\theta;$$

or putting for $\frac{f}{u}$ its value ϖ, the angular velocity round the axe of the impressed moment,

$$G = K\varpi\tan\theta \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (11.)$$

XV. The curve which is the intersection of the ellipsoid and sphere is on a cone of the second degree, whose circular sections are parallel to the central sections of the ellipsoid; let the equations of the ellipsoid and sphere be respectively

$$\frac{x'^2}{a^2} + \frac{y'^2}{b^2} + \frac{z'^2}{c^2} = 1, \text{ and } x'^2 + y'^2 + z'^2 = u^2.$$

Let the equations of the right line, passing through the centre and the point $x' y' z'$, be $x = \frac{x'}{z'} z$, $y = \frac{y'}{z'} z$: by the help of these four equations, eliminating $x' y' z'$, we get

$$\frac{x^2}{a^2} (a^2 - u^2) + \frac{y^2}{b^2} (b^2 - u^2) + \frac{z^2}{c^2} (c^2 - u^2) = 0 \quad . \; . \quad (12.)$$

Hence during the rotation of the body, the axis u of the impressed moment, is found on a cone of the second degree, whose circular sections are parallel to the circular sections of the ellipsoid, as we now proceed to show.

In an ellipsoid, let ϕ be the angle between the plane of a circular section, and the principal plane containing the greatest and mean axe of the ellipsoid, η and ε the eccentricities of the two principal sections perpendicular to this plane, whose semiaxes are b, c and a, c respectively,

$$\text{then } \cos\phi = \frac{\eta}{\varepsilon}, \text{ or } \cos^2\phi = \frac{a^2}{b^2} \frac{(b^2 - c^2)}{(a^2 - c^2)} \quad . \; . \; . \; . \; . \quad (13.)$$

If now in this formula, instead of a^2, b^2, c^2, the squares of the semiaxes of the ellipsoid, we substitute $\frac{a^2}{a^2 - u^2}, \frac{b^2}{b^2 - u^2}, \frac{c^2}{c^2 - u^2}$, which are proportional to the squares of the corresponding semiaxes of the cone, we shall have, calling ϕ' the angle between a circular section of the cone and the plane of $a\ b$,

$$\cos\phi' = \frac{a^2}{b^2} \frac{(b^2 - c^2)}{(a^2 - c^2)},$$

a result independent of u; hence $\phi' = \phi$.

XVI. *The instantaneous axis of rotation moves on a cone of the second degree.*

Assume a point on the perpendicular P, (whose coordinates let be $x\,y\,z$) at the distance l from the centre.

Then $\cos\lambda = \frac{P x'}{a^2}$, or

$$a^4 \cos^2\lambda = x'^2 \left\{ a^2 \cos^2\lambda + b^2 \cos^2\mu + c^2 \cos^2\nu \right\}.$$

Now, $l \cos\lambda = x$, $l \cos\mu = y$, and $l \cos\nu = z$, substituting these values, we find $x'^2 = \frac{a^4 x^2}{a^2 x^2 + b^2 y^2 + c^2 z^2}$.

Finding analogous values for y' and z', introducing the relation

$x'^2 + y'^2 + z'^2 = u^2$, we obtain for the equation of this cone,

$$a^2 (a^2 - u^2) x^2 + b^2 (b^2 - u^2) y^2 + c^2 (c^2 - u^2) z^2 = 0. \quad (14.)^*$$

* It may be worth while, and it is not difficult to show, that the equations of these cones, the loci of the axis of the impressed moment and of

XVII. Now as the axe of the impressed moment is always on the surface of this cone whose circular sections coincide with those of the ellipsoid, the plane of the impressed moment will envelope during the rotation a cone *supplemental** to the former; whose *focals* will be therefore perpendicular to the circular sections of the first cone, that is, to the circular sections of the ellipsoid; hence the *optic axes*† of the ellipsoid of moments will be the focals of the cone enveloped by the plane of the impressed moment; thus the whole motion of the body consists in the uniform rotation of the plane of the impressed moment round its axis, while this plane rolls on the surface of the latter cone.

XVIII. From these considerations it follows, that we may dispense altogether with the Ellipsoid of Moments, and say, *that if two right lines are drawn from the fixed point of the body, in the plane of the axes of the greatest and least moments of inertia, making an angle with the axe of greatest moment whose cosine squared may be equal to* $\frac{\mathrm{A}\,(\mathrm{B}-\mathrm{C})}{\mathrm{B}\,(\mathrm{A}-\mathrm{C})}$, *and a cone be conceived having these lines as focals and touching the plane of the impressed moment, the whole motion of the body will consist in the uniform rotation of this plane section of the body in its own plane, while this plane envelopes the cone.*

Let A C B be the mean section of the ellipsoid, O N′, O N the optic axes, then if the plane of the impressed moment coincides with any of the principal planes, the cones round the optic axes as focals become also planes, and the axes of rotation coincide with the axes of the figure.

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the axis of instantaneous rotation, are equivalent to the equations of the same cones given by Poisson, *Traité de Mécanique*, tom. ii. pp. 151, 152. For this purpose then, assuming the equation given by Poisson, tom. ii. page 140, $\mathrm{A}\,p^2 + \mathrm{B}\,q^2 + \mathrm{C}\,r^2 = h$; and putting for A, B, C, p, q, r, their values given in the preceding pages, we find $h = n^3 f^2$.

Eliminating then from (12.) and (14.) the quantities a, b, c, u, and introducing A, B, C, k, h instead, equation (12.) is changed to

$$\frac{(k^2-\mathrm{A}h)}{\mathrm{A}}x^2 + \frac{(k^2-\mathrm{B}h)}{\mathrm{B}}y^2 + \frac{(k^2-\mathrm{C}h)}{\mathrm{C}}z^2 = 0, \text{ and (14.) is changed to}$$

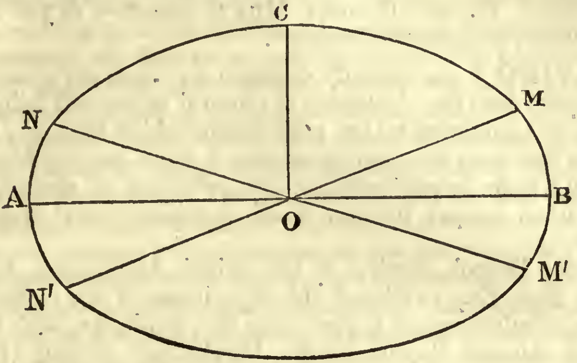
$$(k^2-\mathrm{A}h)\,x^2 + (k^2-\mathrm{B}h)\,y^2 + (k^2-\mathrm{C}h)\,z^2 = 0.$$

* Two cones are said to be supplemental when their corresponding vertical angles are supplemental.

The *focals* of a cone are two right lines drawn through the vertex of the cone, making with the axis through the vertex equal angles, such that the cosine of one of these angles is equal to the ratio of the cosines of the semiangles of the cone.

† I have ventured to use the appellation *optic axes*, a term borrowed from the wave theory of light, to denote the diameters perpendicular to the circular sections of an ellipsoid.

Again, if the plane of the impressed moment intersects the mean plane between N and C, it will envelope the cone whose



focals are O N, O N′; but if the plane of the impressed moment intersect between A and N, it will envelope the cone whose focals are O N, O M; hence the range in the former case, which may be taken as the measure of the stability of rotation round the principal axe whose moment of inertia is greatest, is to the range in the latter, which may be deemed the representative of the stability round the axis whose moment of inertia is the least, as the supplement of the angle between the optic axes, or between the circular sections of the ellipsoid is to the angle itself.

XIX. In the particular case, where the plane of the impressed moment passes through the optic axe of the ellipsoid, the cones become portions of the plane of the greatest and least axes of the surface, divided one from the other by the optic axes; hence if the plane of the impressed moment be disturbed from coincidence with the mean plane of the ellipsoid, so as to commence its rotation round one of the optic axes, it will either return to its former position of coincidence, or revolve through 180° round the optic axe, till it coincides at length with the mean plane on the opposite side; hence the axe of the impressed moment must revolve in a circular section of the ellipsoid, through an angle less than 180°, with a variable velocity, till it at length coincides with the mean axe of the ellipsoid, the body in the mean time uniformly revolving round this axe so moving in the circular section.

[To be continued.]

LXV. *On the Conversion of the Bisulphuret of Copper (Yellow Copper Ore) into the Sulphuret (Vitreous Copper) by Electricity. By Mr.* Robert Hunt, *Secretary to the Royal Cornwall Polytechnic Society**.

HAVING been recently engaged in an experimental inquiry into the electricity of mineral veins, and being desirous of examining all the phænomena which have any bearing on this most interesting subject, I have been induced to institute some experiments on the influence of electric currents upon copper pyrites, which is a bisulphuret of copper and iron.

At the Bristol meeting of the British Association, Robert Were Fox, Esq. exhibited the experiment of the conversion of the bisulphuret of copper into the sulphuret, and in the Fourth Annual Report of the Polytechnic Society (1836), several experiments are detailed in proof of the fact. In repeating those experiments, having adopted other methods than those pursued by Mr. Fox, I have arrived at irrefragable proofs of the process of decomposition, which alone induces me to add my testimony to the high authority of that gentleman.

The original experiment consisted in placing the yellow bisulphuret of copper in a solution of sulphate of copper, divided by a wall of clay from a cell containing plain or acidulated water, in which was placed a piece of metallic zinc connected by a wire with the ore. By this arrangement the ore was in a short time changed, over its surface, into the sulphuret or vitreous copper, having parted with a portion of its sulphur and its iron.

1. I was extremely desirous of ascertaining if this change would take place in a solution which did not contain copper. I therefore divided a vessel by a thin wooden partition. In one cell I placed a solution of the sulphate of soda, and in the other water acidulated with sulphuric acid. Into the sulphate of soda I placed a piece of the bisulphuret of copper weighing two hundred and fifty-six grains, connected with a piece of zinc which dipped into the other cell. After three days, the ore having become first iridescent and then gray over its surface, I removed and weighed it. Its weight was now two hundred and forty-seven grains, *having lost nine grains*. By adding ferrocyanate of potash to the solution a considerable portion of the cyanate of iron was formed; a convincing proof of the separation of iron from the ore.

2. A piece of the copper pyrites, weighing two hundred

* Communicated by the Author.

grains, was placed in a solution of the *muriate of barytes*, water only being in the cell containing the zinc with which the ore was connected. In a few days the ore was covered with a reddish-brown powder, which was proved to be peroxide of iron, beneath which was a coating of vitreous copper. A quantity of the *sulphate of barytes* was formed and collected, and traces of copper were evident in the solution. During the process there was a steady liberation of gas from the fluid in contact with the copper ore, which was collected and found to be pure hydrogen. This experiment proves that the sulphur liberated from the yellow ore is converted into sulphuric acid at the expense of the oxygen of the water, which is seized by the barytes and forms the sulphate of that earth, and that the iron is freed in the state of peroxide. It is possible the copper detected in this instance may have arisen from the wire used to make the connexion. The water in the cell which contained the zinc was now found, as was of course expected, to hold muriate of zinc in solution. From the adhesion of the peroxide of iron to the piece of ore, it was difficult to ascertain the actual loss of weight it had suffered.

3. Two pieces of copper pyrites, each weighing seventy-two grains, were connected by *zinc wires* with a single pair of zinc and copper plates excited by acidulated water, and both plunged into a solution of the muriate of soda. The zinc wires were so quickly destroyed from the action of the electro-negative ore on the positive metal, that it was found impossible to carry on the experiment for any time. The change was, however, well marked even in this case.

4. Seventy-four grains of copper pyrites was connected by *silver wire*, with the positive pole of a circular battery of a single pair excited with salt and water, and fifty-two grains of the same ore with the negative pole in like manner. These two pieces were placed in a precipitating glass filled with a solution of the muriate of barytes. It will be evident, that in this experiment the fragments of ore form the poles of a galvanic arrangement. In forty-eight hours they were removed; it was now found that one piece weighed seventy-five grains, and the other only fifty, the latter being partially changed into vitreous ore, while the former was covered with specks of the *peroxide of iron*. It was extremely interesting to find the peroxide acting as an acid would have done, and passing to the positive pole. Rather more than a grain of *sulphate of barytes* was collected from the glass; and the solution on being treated with the ferrocyanate of potash gave a precipitate of the *cyanate of copper*: copper in minute quantity was also detected by means of a piece of polished iron.

These experiments appear to me sufficient to prove the decomposition of the yellow ore. The loss of weight which is detected is accurately made up by the peroxide of iron, the sulphur of the sulphate of barytes and the copper. It may appear to some that the alterations in weight in the last experiment (4.) are trifling, but it must be remembered that the copper pyrites was exposed, for a short time only, to a weak current from a rather inconstant battery. If the action had been continued longer, with a sustaining battery the loss of weight would have been proportionally greater.

5. With a view of ascertaining the effect of a more powerful current on similar pieces of ore, I placed one piece in a solution of the sulphate of copper, and another in water divided from it by a membrane, connecting them respectively with the positive and negative poles of a battery of twelve pairs arranged as the *couronne de tasses*, excited with strong brine. I thus procured a tolerably powerful, but constantly weakening electric current. The arrangement was left for two days, when the piece of ore in the solution of sulphate of copper, which was connected with the negative pole, was found to have crumbled down considerably, eight grains of vitreous copper in a state of fine division having been collected from the bottom of the vessel, the loss of weight on the whole being ten grains and a half. Nothing is more interesting than to observe, on the grand scale of natural operations, examples of processes which we have imitated in the laboratory, and thus to find, at the same time, a proof of the correctness of our experiments, and a complete elucidation of a natural phænomenon.

At the last annual meeting of the Polytechnic Society, a communication was read from Joseph Carne, Esq., stating, that at the mines in the neighbourhood of Marazion, the bisulphuret of copper was always found in the blue slate, whereas the vitreous ore was constantly found in the red slate; and that the same fact was observed at the Carn Brea mines near Redruth, where the yellow ore was invariably changed into vitreous ore whenever the granite exhibited a red colour. This tincture is due to the peroxide of iron; and it is worthy of remark, that the stain does not extend many feet on either side of the load.

I must be excused from remarking, that it appears to me evident that the change of the bisulphuret of copper into the sulphuret is explained in the preceding experiments, and shown to depend upon the decomposing power of the electric currents which circulate through the metalliferous veins, as was discovered by Mr. Robert Fox, and the decomposing

agency of which I have proved much to my satisfaction in some experiments conducted in East Pool copper mine.

November 4, 1841.

LXVI. *On the History of* Fernel's *Measure of a Degree.* *By Professor* De Morgan.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to call the attention of mathematicians to a very strange circumstance in the account of Fernel's measure of a degree, which will show how the history of science is sometimes written.

Montucla (vol. ii. p. 231, first edition; vol. ii. p. 316, second edition) gives the usual account of Fernel's measuring by the revolutions of a wheel from Paris towards Amiens, and finding a degree to be 56746 Paris toises. In the second edition he adds, that the details are found in Fernel's *Cosmotheoria.*

Lalande (*Bibliographie Astron.*, p. 46) gives the *Cosmotheoria* as having 46 *feuillets*, and as published in 1528: he adds, that it is remarkable as containing the first determination of the true magnitude of the earth, and refers to his own account of it in the Memoirs of the Academy for 1787, page 216.

Delambre (Astronomy, vol. iii. p. 516) repeats the story, but makes the result 57070 toises: he does not refer to the *Cosmotheoria.*

On looking over the work of Fernel for another purpose, I was surprised to see that he himself states a very different result, and piques himself on its close agreement with that of Almæon (Al Mamun he means). The story about the wheel is correctly told; it should have been added, that he observed his solar altitudes with Ptolemy's rules. His result is, that the degree of latitude consists of 68 *Italian* miles and 96 paces, which he makes 68 miles 95$\frac{1}{4}$ paces, to avoid fractions in the resulting diameter of the earth. I quote three different places where he has expressed this result, premising that my copy has 46 leaves, and was published in 1528; so that all doubt of its being the work described by Lalande may be impossible.

Leaf 2, page 1. " ..., idipsum experimento comprobans, deprehendi accurata supputatione, cuique gradui circuli majoris tam in terræ quàm in maris convexo 68 Italica miliaria, passus 95 cum una quarta respondere."

Leaf 2, page 2. "Cuivis gradui ambitus terræ 68 milliaria, 95¼ passus."

Leaf 3, page 2. "Reperi passus 68096 qui milliaria sunt Italica 68, cum passibus 96. Malui tamen hos passus, in passus 95 cum uno quarto convertere, ne quæpiam fractio foret in terræ diametro præfigenda."

Now let us ask what are 68·096 Italian miles? The Italian mile was always used, under the idea that it was the old Roman mile; from which in fact it differs little for our present purpose, the latter being about 1614 yards, and the former 1628. But not to take any point against Fernel's measure, let us follow Dr. Bernard, who makes the statute mile to be 1056 paces, which would give 1667 yards to the mile of 1000 paces. This would give 64½ English statute miles to Fernel's degree, about *five miles* too little. And if, as is more probable, we take 1628 yards for Fernel's Italian mile, we find only 63 statute miles for his degree. The French toise is 2·1315 yards; so that Delambre makes Fernel's degree to be 69·12 miles. How he made the Italian mile longer* than the English statute mile, I have no idea. The confusion is older than Delambre and Lalande, as appears by Montucla's first edition. Riccioli gives a true account and so does Dr. Bernard. Weidler (p. 341) omits all mention of the degree of the *Cosmotheoria*, but says (without citing authority) that Fernel undertook to measure a degree, which he finished *in the year* 1550. The copy from which I quote belonged to Montucla, who has corrected errors in several places, but has taken no notice of this, though I see from one of his marks that he had read the page. Did Fernel measure another degree in 1550? If so, what is the authority for it? Why did Montucla, who is proved to have seen this very page of Weidler, refer to the *Cosmotheoria*? And why is this second degree mentioned only by Weidler? Perhaps some of your readers may be able to clear up this point.

Richard Norwood (Seaman's Practice, p. 41) being naturally desirous to get confirmation of his own degree (which I cannot help thinking has been much underrated) first contrives to twist the Arabian degree into nearly the same as his own; and then allows Fernel's to come as near to it, but only by representing (without authority) "the pace which he used as being more than five of our English feet." Yet Norwood himself, in the very first sentence of his own work, says, "It is

* There is a Tuscan and Lombard mile in modern road-books, which is a trifle longer than the English mile: but Delambre never could have mistaken this for the famous Italian mile, the universal standard of the middle ages.

a commonly received opinion, that allowing five of our English feet to a geometrical pace, a thousand of these paces make an Italian mile;" and Fernel certainly used the Italian mile of his day. On this the whole question seems to turn, but I think I may confidently assert that no evidence can be brought to show that the curious fiction which geometers of the sixteenth century called a geometrical pace was even as long as five English feet; and certainly it was not longer.

I remain, Gentlemen, yours faithfully,

University College, Oct. 26, 1841. A. De Morgan.

LXVII. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 285.]

On Succino-hyposulphuric Acid.

BY the action of anhydrous sulphuric acid on benzoic acid, Mitscherlich prepared his benzoesulphuric acid (benzoehyposulphuric acid, Fehling). Mitscherlich's formula is $S^2 O^6, C^{14} H^{10} O^3 + H^2 O$. Fehling gives the formula, according to Liebig's theory of the constitution of organic acids, as $S^2 O^5, C^{14} H^8 O^3 + 2 H^2 O$. Fehling has succeeded in preparing a similar compound with succinic acid. He treated succinic acid with the vapour of anhydrous sulphuric acid; it is absorbed in large quantities, and heat is evolved; a brown elastic mass is formed, which must be allowed to stand for twenty-four hours. The mass is then diluted with water, and carbonate of baryta or lead added until the filtered solution gives no precipitate with chloride of barium. The filtered solution is then precipitated by acetate of baryta or lead, and from the edulcorated salt of lead the free acid may be prepared by means of sulphuretted hydrogen. The solution evaporated *in vacuo* gives small verrucose crystals, which cannot be perfectly dried. The acid is soluble in water and alcohol. These crystals, when heated with carbonate and nitrate of potassa, gave 99·21% sulphate of baryta; according to the formula $C^8 H^4 O^5 + S^2 O^5 + 8 H^2 O$, the quantity would be 99·2%. The dry acid is probably $C^8 H^4 O^5 + S^2 O^5 + 4 H^2 O$. From the composition of the salt of lead, it is probable that the acid is quadribasic; $2 (C^4 H^4 O^3 + H^2 O) + 2 S O^3 = C^8 H^4 O^5 + S^2 O^5 + 4 H^2 O$. By completely saturating the acid with potash, a very deliquescent salt is obtained; by the addition of a little acid to its solution a salt is formed which may be obtained in good crystals, and is not deliquescent; it contains six atoms of water; *in vacuo* it loses two atoms. This salt with four atoms loses at 100° three atoms

more, and its formula is then $C^8 H^4 O^5, S^2 O^5 + 3 KO + H^2 O$, or $C^8 H^4 O^5 . \underline{\dddot{S}} + \left.\begin{matrix} 3 \dot{K} \\ \underline{\dot{H}} \end{matrix}\right\}$.

The salt dried *in vacuo*, and then at a gentle heat, contains three atoms of water, $C^8 H^4 O^5, S^2 O^5 + 3 KO + 3 H^2 O$. By adding more acid to this succino-hyposulphate another salt is obtained, $C^8 H^4 O^5, S^2 O^5 + 2 KO + 6 H^2 O$. It loses one atom of water *in vacuo*, at a gentle heat another atom, and at 100° two more. The ammonia salt dried at a low temperature is $C^8 H^4 O^5, S^2 O^5 + 3 N^2 H^8 O + 3 H^2 O$. The baryta salt is insoluble; its formula, after being dried at 100°, is $C^8 H^4 O^5, S^2 O^5 + 3 Ba O + H^2 O$. The lime salt is uncrystallizable; it contains two atoms of lime. If the acid solution, which has been filtered from the sulphate of lead in the preparation of the acid, be precipitated by acetate of lead, a salt falls containing 3 Pb O; if the solution be first neutralized with ammonia, the precipitate contains 4 Pb O. The first salt when dried in the air is $C^8 H^4 O^5, S^2 O^5 + 3 PbO + 4 H^2 O$. It loses three atoms of water at 100° C. The salt with 4 Pb O contains four atoms of water, which are driven off at 100° C. Formula $C^8 H^4 O^5, S^2 O^5 + 4 Pb O$. The silver salt is very easily decomposed.

In the formation of benzoe-hyposulphuric acid, Liebig supposes that one atom of oxygen derived from the two atoms of sulphuric acid, combines with two atoms of hydrogen derived from the benzoic acid; the water thus formed adds itself to the basic water of the benzoic acid, and the resulting compound is therefore *bibasic*. Berzelius supposes that the water is derived from the benzoic acid alone, which is thus converted into a substance without acid properties, which can combine with sulphuric acid without influencing its saturating power. Mitscherlich also believes that the saturating power depends wholly on the inorganic acid. But the succino-hyposulphuric acid saturates four atoms of base; if the atom of succinic acid be $C^4 H^4 O^3 + H^2 O$, then both acids have retained their saturating power in the new compound, and, from analogy, benzoesulphuric acid ought to be tribasic. Fehling considers it most probable that succinic acid is a tribasic acid; the hypothetical anhydrous acid is the $C^8 H^6 O^5$; this combines with two atoms of sulphuric acid, one atom of water is given off, and hyposulphuric acid, which saturates only one atom of base, is formed. The compound acid will then be quadribasic. Crystallized succinic acid is probably $C^8 H^6 O^5 + 3 H^2 O$. (*Annalen der Chemie und Pharmacie*, xxxviii. p. 285.)

On the Compounds of Cyanogen with Sulphuretted Hydrogen.

Völckel has made some experiments, under Wöhler, on the two compounds obtained by mixing together cyanogen and sulphuretted hydrogen. The yellow compound has been described by Gay-Lussac, the red one by Wöhler. By passing the two gases at the same time into alcohol, but so that the cyanogen is always in excess, the yellow body is obtained; when sulphuretted hydrogen is in excess the red one is formed. They may be purified by recrystallization out of alcohol. The red body has the formula $C^2 N^2 H^4 S^2$. By adding acetate of lead to an excess of this body dissolved in alcohol, a yellow precipitate is obtained; its formula, when dried *in vacuo*, is $C^2 N^2 H^2 S^2 Pb$. In the red body, therefore, one equivalent of hydrogen has been replaced by one equivalent of lead. It may be considered as similar to mercaptan, viz. $C^2 N^2 H^2 S + H^2 S$. The lead compound is $C^2 H^2 N^2 S + Pb S$. By boiling with water this salt is decomposed, sulphuret of lead is formed, cyanogen is evolved, and the solution contains both the red and the yellow body. The red body is soluble in a *cold* solution of potassa, it is precipitated again by acids. Boiled with a *concentrated* solution of potassa, it forms sulphocyanuret, sulphuret, and cyanuret of potassium. Boiled with a dilute solution it forms ammonia, oxalate of potassa, and sulphuret of potassium. It may be considered as oxamid, in which the oxygen is replaced by sulphur. The yellow compound decomposes very easily, does not give any lead compound; with a salt of silver it is decomposed like the red compound; cyanogen is evolved, and sulphuret of silver precipitated. According to Gay-Lussac, its composition is $C^4 N^4 H^6 S^3$, or perhaps $2\ C^2 N^2 H^2 S + H^2 S$. If we double its formula we have the composition of allantoin, in which the oxygen is replaced by sulphur. (*Annalen der Chemie und Pharmacie*, xxxviii. p. 314.)

On some Benzoyl Compounds.

Laurent has examined the action of sulphuretted hydrogen and of hydrosulphuret of ammonium on the benzoyl compounds.

Hydruret of sulphobenzoyl is obtained by dissolving one volume of oil of bitter almonds in eight or ten volumes of alcohol, and adding slowly one volume of the hydrosulphuret; after a few minutes a white mealy powder is deposited. By adding a little hydrosulphuret to the boiling hot alcoholic solution of the oil, a white voluminous precipitate of the hydruret is produced. It is insoluble in water and alcohol,

is dissolved by æther, but only in small quantity; becomes soft at 90° or 95° C. By distilling it is decomposed into several new bodies; it is decomposed by nitric acid. Formula $C^{14} H^{12} S^2$. The oxygen of the benzoyl has therefore been replaced by sulphur. [Might not the formula be $C^{14} H^{10} S + H^2 S$, *i. e.* hydrosulphuret of benzoyl? Berzelius assumes a radical in benzoic acid, $C^{14} H^{10}$, which he calls benzoyl.—H. C.]

Hydruret of sulphazobenzoyl is formed with the above, but only in small quantity. It may be obtained by mixing one volume of hydrosulphuret of ammonium with a solution of one volume of oil of bitter almonds in four or five volumes of æther, and allowing the mixture to stand for a month; a white crystalline crust is formed, which may be purified by resolution in æther, &c. This hydruret is colourless and transparent, may be obtained in good crystals, oblique prisms with rectangular base. It melts at 125°; at a higher temperature ammonia is evolved and other products are formed. Boiling alcohol slowly decomposes it. It is rapidly decomposed by warm nitric acid; an oil (bitter almond oil?) is formed. Formula $C^{126} H^{108} N^4 S^{12}$, or six atoms of hydruret of sulphobenzoyl, and two atoms of hydrobenzamid (hydruret of azobenzoyl), $6 (C^{14} H^{10} S^2 + H^2) + 2 (C^{21} H^{15} N^2 + H^3)$.

Hydrosulphate of Azobenzoyl.—Equal volumes of almond oil, ammonia, and hydrosulphuret of ammonium, are allowed to stand six months in a closed bottle, the oil becomes almost solid; on boiling the mass with æther, a small quantity of the hydrosulphate remains in form of a white powder. It is almost insoluble in alcohol, somewhat soluble in æther, and from this solution it may be obtained in microscopic crystals. The probable formula is $C^{14} H^{10} N^2 + H^2 S$.

Azobenzoidin.—Oil of bitter almonds, which had not been purified by means of protochloride of iron and potassa, was distilled, and the first two-thirds preserved. A part of this, mixed in a bottle with an equal volume of ammonia, became a solid mass in fourteen days; this was treated with cold æther, which dissolved an oily matter, a substance crystallizing in needles and azobenzoyl: a white powder remained, which, when boiled with æther, left some benzoylazotid behind mixed with another substance, and the æthereal solution gave on evaporation crystals of azobenzoidin. It is colourless and inodorous, almost insoluble in alcohol, somewhat soluble in æther, is decomposed by boiling hydrochloric acid, forms with boiling nitric acid an acid probably new. Formula, $C^{14} H^{11} N^{\frac{2}{3}}$. If this formula be doubled, and fifty-nine

taken as the equivalent of nitrogen, we obtain the formula $(C^{14} H^{10} N^2 + N) + (C^{14} H^{10} N^2 + H^2.)$ This body is isomeric with azobenzoid, which is insoluble in æther, uncrystalline, but crystallizes after fusion, which the above body does not.

Azobenzoïlid.—Purified bitter almond oil was distilled, and the first eighth of the product mixed with ammonia; in three weeks half of the oil had become solid; æther extracted the oily portion, the remainder was crystalline. It is insoluble in alcohol, very little soluble in æther, is decomposed by distillation, forms with nitric acid an oily substance. Formula $C^{14} H^{11} N^{\frac{5}{3}}$. It is therefore isomeric with azobenzoid and azobenzoidine.

Hydruret of benzoyline is formed by treating hydruret of azobenzoylin with hydrochloric acid; a thick oily matter is produced, which must be washed with boiling water and dried until it is solid. It is colourless, inodorous, easily soluble in alcohol and æther, is not decomposed by distillation; forms a compound with bromine. By the addition of ammonia to its boiling alcoholic solution, hydruret of azobenzoylin is formed. Formula $C^{14} H^{12} O^2$. It is therefore isomeric with hydruret of benzoyl.

Hydruret of Azobenzoylin.—It has been mentioned above, that in the process for procuring azobenzoidin by the action of ammonia on almond oil, an oily matter, azobenzoyl, and a substance in acicular crystals are produced. This mixture is heated with alcohol until it boils, and hydrochloric acid added; the whole is dissolved, except a small quantity of a crystalline substance. The alcoholic solution deposits an oily matter on evaporation; this, when treated with ammonia, becomes solid in a few minutes. The solid part must be quickly washed with a mixture of alcohol and æther, by which a white substance is obtained, which may be purified by repeated recrystallization out of alcohol. It is colourless, inodorous, tolerably soluble in boiling alcohol; the solution deposits it on cooling in the form of long needles. It may be distilled without decomposition, but is easily decomposed by hydrochloric acid. Formula $C^{14} H^{12} N^{\frac{4}{3}}$. It is therefore isomeric with hydrobenzamid, benzhydramid, and benzoinamid. The hydrobenzamid is easily soluble in æther, and crystallizes in octahedrons. Benzhydramid is not decomposed by hydrochloric acid, and benzoinamid is distinguished by its almost perfect insolubility.

Laurent has discovered benzoyl (Liebig's benzil) in the purified oil of bitter almonds. By treating almond oil with

chloride of sulphur, benzoate of hydrobenzoyl is produced. (*Annales de Chemie et de Phys.*, 3rd ser., t. i. p. 291.)

[It may be remarked, that the mention of a great number of other bodies, whose composition and properties have not been examined, has been excluded from this extract.]

Formation of Murexid.

Liebig and Wöhler have found that when a *concentrated* solution of *pure* alloxan is boiled, an evolution of carbonic acid takes place, which continues for a long time. The solution then gives, with baryta, a blue precipitate, and with carbonate of ammonia a considerable quantity of murexid. On cooling, and during the boiling, a quantity of alloxantin is deposited, which explains the formation of murexid. If the mother liquor is saturated with ammonia, oxalurate of ammonia is obtained. Three atoms of alloxan give

$$\begin{array}{ll}
\text{2 atoms Alloxantin} & = C^{16}\,H^{20}\,N^{8}\,\;O^{20}. \\
\text{1 atom Parabanic acid} & = C^{6}\;\;H^{4}\;\;N^{4}\;\;O^{6}. \\
\text{2 atoms Carbonic acid} & = C^{2}\qquad\qquad\quad O^{4}. \\
\hline
\text{3 atoms Alloxan} & = C^{24}\,H^{24}\,N^{12}\,O^{30}.
\end{array}$$

(*Annalen der Chemie und Pharmacie*, xxxviii. p. 357.)

LXVIII. *A few Remarks on Electro-Metallurgy.* By Mr. T. B. Jordan*.

IN the course of a limited series of experiments, which I have tried in this interesting branch of the useful application of electricity, a few ideas have occurred to me, which, I believe, possess the charm of novelty. As no suggestion, however trivial in itself, can be considered altogether unimportant when it bears on the practical application of science to our manufacturing arts, I feel no hesitation in laying before the Society the following remarks with a view to their being made public. It is quite evident that Mr. Spencer's recent discovery, of the art of depositing metals from a solution of their salts by galvanic action, has placed in the hands of manufacturers a new means of copying, and the importance of this will be immediately apparent to those who know how large a portion of our manufactures in metal are the copies of original matrixes, or more indirectly the results of other copying processes. Some imagine that because it is only the means of copying, it is really of little value; but it is impossible to allow this to be a valid objection, when we consider how very few metal articles there are, which do not wholly or

* From the Eighth Annual Report of the Royal Cornwall Polytechnic Society, 1840.

in part owe their origin to copying. If then it can be shown that this method possesses advantages for some description of work, which are not common to any other, and that there is good reason to suppose it may be œconomically conducted, then we may fairly infer, that it will in due time become one of the ordinary processes of our factories.

Its advantages are numerous, and some of them so self-evident, that they only require naming to be allowed; for instance, any surface, whatever be its material or form, may be covered with a coating of metal, without heat and without force; this coating may be allowed to accumulate to the required thickness, and may then be removed from its original, when the surfaces which were in contact will be found to be so perfectly alike, that it is impossible to discover the minutest scratch in one which has not its counterpart in the other. In proof of the accuracy of the copy, I may refer to the numerous admirable copies of engraved plates and medals already before the public; if any of these specimens be examined with a glass, it will be found that not only every line of the graver, but every scratch in the polished surface of the original plate is faithfully transferred, both to the deposited matrix, and the subsequent copy of it; and when we consider the fact that both these copies are of the same material as the original plate, and that that may just as well have been of a much softer and more workable material, we must immediately conclude that there is no other known mode of copying possessing like advantages. But as the application of it to the copying of engravings, medals, woodcuts, and similar subjects, is already well known, I merely refer to it in illustration of my views, and will now offer a few reasons for supposing that it may eventually be found an œconomical process for some of the more ordinary branches of our manufactures in metal.

I can now only explain the principles which lead me to imagine that it would not be a costly process on the large scale. In my preliminary experiments, I have used Mr. Smee's battery of platinated silver and zinc, excited by diluted sulphuric acid; it acts well, and is more convenient than the sulphate of copper batteries.

Mr. Spencer's very simple and cheap arrangements, in which the negative plate of a single pair is that receiving the deposit, is I think very suitable for flat work, but for complicated forms I should certainly prefer having the battery arrangement separate from the depositing cell, both on account of its being more manageable, and because in this way the solution of the sulphate of copper is kept constantly at the

point of saturation by the destruction of a portion of the copper plate connected with the negative plate of the battery; of course the solution of this plate is effected by the portion of acid set free by depositing its copper on the mould, assisted by the galvanic action; so that in fact this method gives us the power of removing the material from any old and irregular piece of metal, and depositing it in the most highly wrought mould of which we may wish to obtain a copy.

In taking a comparative view of the probable cost of production, it will not be necessary to consider the first outlay for apparatus and moulds, because there must be a much larger outlay before commencing operations in the usual way; and leaving this out of the question, the cost of any electro-deposit would be made up of the value of the raw material, of the zinc and acid used in the battery, and of the time employed in making the arrangement.

The raw material may be obtained in considerable quantities, at its lowest price, as old sheathing copper would be admirably adapted to the purpose, on account of the facility with which it may be bent to any required form. The zinc and acid used in the battery would only have combined to form sulphate of zinc, which would be of some value. The time required for making the arrangements in the different troughs of a large factory, would be comparatively small, because one man may keep a great number at work, as they only require attention once in twenty-four hours—of course there must be many other persons employed in such a factory, in preparing new patterns, and new battery plates and connexions, but these come under other heads of expenditure which have their counterparts in the present mode.

I will now suggest a few of the plans which may be used for producing hollow vessels. The most simple process which has occurred to me, and the one which I have already used with success, is to form a block of some easily fused material into the shape of the interior of the required vessel, which serves as the matrix for depositing the copper on; when this is sufficiently thick, it may be placed on the fire till the block is melted, when the fluid material may be poured from any of its openings, and leave the hollow vessel complete and in a single piece, without the aid of the soldering iron or hammer. The temporary block on which this vessel is deposited may be formed of wax, cement, fusible metal, lead, or any other convenient material which melts at a much lower temperature than the metal to be deposited. I consider this method to be peculiarly applicable to the most complicated forms of metallic musical instruments, and to crooked tubular

work generally; but it has certainly the great disadvantage of requiring a new mould for every repetition of the work, *or rather a new block*, for these may in many cases be cast in a mould which would be permanent for any number of repetitions. There is however one means of avoiding this in some cases, which is very well illustrated by the common *boot-tree*; if this were put together, and after having its surface metalized, placed in the depositing trough for a proper time, we should certainly have it covered with copper; we may then take out the centre or key-piece, which would unlock the others, and they may readily be removed, leaving a copper boot—not a very desirable article certainly, but the block may just as well have been formed to a more suitable shape. Still there are disadvantages in this mode of procedure which would limit the application to a few articles, although there are some which could not readily be accomplished in any other way. For the production of ornamental vases and other works of value, I would suggest the following method:—Let the whole, or a segment of the work, according to the nature of the design, be moulded in wax, carved in wood, cast in plaster, or produced in any more convenient way; then metalize the surface, and deposit on as large a portion of the original as will relieve in one piece; repeat this at different times over the whole work, and so manage the edges of each piece that the whole shall key together in the usual way, so that when finished you will possess a metallic mould for the required work. Should the design be complicated in form, a number of pieces will be requisite; but there are many very expensive articles which would readily relieve if the mould were in a single piece, and a very much more extensive class which may be made in two parts. Of course the inside of these metallic segments would correspond in polish and finish to the original model, and the metal mould, when complete, would serve for an unlimited number of copies, which would not require any cleaning up or hand finishing, but the parts would require to be put together, which I propose to effect by the same power that produced them; this I think may be done by covering the work with wax varnish while warm, and afterwards carefully cleaning all the edges, and then binding it together as if for soldering; the joints must now be touched over with nitric acid, and the article properly connected with the battery, and placed in the depositing trough; copper will immediately begin to deposit on the joints, and in a few hours I imagine that these joints would be quite as strong as any other part; if so, the method would have the decided advantage of joining the work with pure copper, and of doing this without ex-

posing any part of it to the fire. I have not yet attempted to join any work in this way, but I consider that I have good evidence of its practicability, in the well-known fact of deposited copper uniting with any other portion of copper which has been cleaned with nitric acid, quite as intimately as the different parts of the original piece are united.

In the foregoing remarks I have endeavoured to show, that the system of electro-metallic deposits is capable of producing every article which leaves the workshop of the coppersmith, and that there is no very apparent reason for supposing that it would be too costly to compete with the usual processes of manufacture; still much of this opinion is speculative, and as yet I have not sufficient experimental evidence to offer in support of it; but such as I have, has fully satisfied me that it is correct. The small toy which accompanied this paper, was made a few days before the meeting, and I believe it is the first symmetrical copper vessel ever produced by galvanic action.

LXIX. *On the Chemical Statics of Organized Beings.* *By* M. Dumas.

[Continued from p. 347, and concluded.]

III.—LET a seed be thrown into the earth, and be left to germinate and develop itself; let the new plant be watched until it has borne flowers and seeds in its turn, and we shall see by suitable analyses, that the primitive seed, in producing the new being, has fixed carbon, hydrogen, oxygen, azote and ashes.

Carbon.—The carbon originates essentially from carbonic acid, whether it be borrowed from the carbonic acid of the air, or proceed from that other portion of carbonic acid which the spontaneous decomposition of manures continually gives out in contact with the roots.

But it is from the air especially that plants most frequently derive their carbon. How could it be otherwise when we see the enormous quantity of carbon which aged trees, for example, have appropriated to themselves, and yet the very limited space within which their roots can extend? Certainly, when a hundred years ago the acorn germinated, which has produced the oak that we now admire, the soil on which it fell did not contain the millionth part of the carbon that the oak itself now contains. It is the carbonic acid of the air which has supplied the rest, that is to say, nearly the whole.

But what can be clearer and more conclusive than the experiment of M. Boussingault, in which peas, sowed in sand, watered with distilled water, and having no aliment but air, have found in that air all the carbon necessary for development, flowering and fructification?

All plants fix carbon, all borrow it from carbonic acid, whether this be taken directly from the air by the leaves, whether the roots imbibe within the ground the rain water impregnated with carbonic acid,—or whether the manures, whilst decomposing in the soil, supply carbonic acid, which the roots also take possession of to transmit it to the leaves.

All these results may be proved without difficulty. M. Boussingault observed that vine leaves which were inclosed in a globe, took all the carbonic acid from the air directed across the vessel, however rapid the current. M. Boucherie also observed enormous quantities of carbonic acid escape from the divided trunk of trees in full sap, evidently drawn by the roots from the soil.

But if the roots imbibe this carbonic acid within the earth, if this passes into the stalk and from thence into the leaves, it ends by being exhaled into the atmosphere, without alteration, when no new force intervenes.

Such is the case with plants vegetating in the shade or at night. The carbonic acid of the earth filters through their tissues and diffuses itself in the air. We say that plants produce carbonic acid during the night; we should say, in such a case, that plants transmit the carbonic acid borrowed from the soil.

But let this carbonic acid, proceeding from the soil or taken from the atmosphere, come into contact with the leaves or the green parts, and let the solar light moreover intervene, then the scene all at once changes.

The carbonic acid disappears; bubbles of free oxygen arise on all the parts of the leaf, and the carbon fixes itself in the tissues of the plant.

It is a circumstance well worthy of interest, that these green parts of plants, the only ones which up to this time manifest this admirable phænomenon of the decomposition of carbonic acid, are also endowed with another property not less peculiar, or less mysterious.

In fact, if their image were to be transferred into the apparatus of M. Daguerre, these green parts are not found to be reproduced there; as if all the chemical rays, essential to the Daguerrian phænomena, had disappeared in the leaf, absorbed and retained by it.

The chemical rays of light disappear, therefore, entirely in

the green parts of plants; an extraordinary absorption doubtless, but which explains without difficulty the enormous expense of chemical force necessary for the decomposition of a body so stable as carbonic acid.

What, moreover, is the function of this fixed carbon in the plant? for what is it destined? For the greater part, without doubt, it combines with water or with its elements, thus giving birth to matters of the highest importance for the vegetable.

If twelve molecules of carbonic acid are decomposed and abandon their oxygen, the result will be twelve molecules of carbon, which, with ten molecules of water, may constitute either the cellular tissue of plants, or their ligneous tissue, or the starch and the dextrine which are produced from them.

Thus in any plant whatever, nearly the entire mass of the structure (charpente), formed as it is of cellular tissue, of ligneous tissue, of starch, or of gummy matters, will be represented by twelve molecules of carbon united to ten molecules of water.

The ligneous part, which is insoluble in water,—the starch, which gelatinizes (*l'amidon, qui fait empois*) in boiling water,—and the dextrine, which dissolves so easily in water cold or hot, constitute therefore, as M. Payen has so well proved, three bodies possessing exactly the same composition, but diversified by a different molecular arrangement.

Thus, with the same elements, in the same proportions, vegetable nature produces either the insoluble walls of the cells of cellular tissue, and of the vessels, or starch which she accumulates as nourishment around buds and embryos, or the soluble dextrine which the sap can convey from one place to another for the wants of the plant.

How admirable is this fecundity, which out of the same body can make three different ones, and which allows of their being changed one into the other, with the slightest expense of force, every time occasion requires it!

It is also by means of carbon united with water, that the saccharine matters so frequently deposited in the organs of plants for peculiar purposes, which we shall shortly mention, are produced. Twelve molecules of carbon and eleven molecules of water form the cane sugar. Twelve molecules of carbon and fifteen molecules of water make the sugar of the grape.

These ligneous, amylaceous, gummy and saccharine matters, which carbon, taken in its nascent state, can produce by uniting with water, play so large a part in the life of plants, that,

when they are taken into consideration, it is no longer difficult to understand the important part that the decomposition of carbonic acid performs in plants.

Hydrogen.—In the same manner that plants decompose carbonic acid for the appropriation of its carbon, and in order to form together with it all the neutral bodies which compose nearly their entire mass, in the same way, and for certain products which they form in less abundance, plants decompose water and fix its hydrogen. This appears clearly from M. Boussingault's experiments on the vegetation of peas in closed vessels. It is still more evident from the production of fat or volatile oils so frequent in certain parts of plants, and always so rich in hydrogen. This can only come from water, for the plant receives no other hydrogenated product than the water itself.

These hydrogenated bodies, to which the fixation of the hydrogen borrowed from the water gives birth, are employed by plants for accessory uses. They form indeed the volatile oils which serve for defence against the ravages of insects; fat oils or fats, which surround the seed, and which serve to develop heat by oxidation (*en se brûlent*) at the moment of germination; waxes with which leaves and fruits are covered so as to become impermeable to water.

But all these uses constitute some accidents only in the life of plants; thus the hydrogenated products are much less necessary, much less common in the vegetable kingdom than the neutral products formed of carbon and water.

Azote.—During its life, every plant fixes azote, whether it borrows the azote from the atmosphere, or takes it from the manure. In either case it is probable that the azote enters the plant and acts its part there only under the form of ammonia or of nitric acid.

M. Boussingault's experiments have proved that certain plants, such as Jerusalem artichokes, borrow a great quantity of azote from the air; that others, such as wheat, are on the contrary obliged to derive all theirs from manure; a valuable distinction for agriculture; for it is evident that all cultivation should begin by producing vegetables which assimilate azote and air, to rear by their aid the cattle which will furnish manure, and employ this latter for the cultivation of certain plants, which can take azote from the manures only.

One of the most interesting problems of agriculture consists then in the art of procuring azote at a cheap rate. As for carbon, no trouble need be taken about it; nature has provided for it; the air and rain water suffice for it. But the azote of the air, that which the water dissolves and brings with it; the

ammoniacal salts which rain water itself contains, are not always sufficient. With regard to most plants, the cultivation of which is important, their roots should also be surrounded with azotated manure, a permanent source of ammonia or of nitric acid, which the plant appropriates as they are produced. This, as we know, is one of the great expenses of agriculture, one of its great obstacles, for it possesses only the manure which is of its own production. But chemistry is so far advanced in this respect, that the problem of the production of a purely chemical azotated manure cannot be long in being resolved.

M. Schattenman, the skilful director of the manufactories of Bouxvilliers in Alsace, M. Boussingault and M. Liebig, have turned their attention to the functions of ammonia in azotated manures. Recent trials show that the nitric acid of the nitrates also merit particular attention.

But for what purpose is this azote, of which plants seem to have such an imperious want? M. Payen's researches partly teach us, for they have proved that all the organs of the plant, without exception, begin by being formed of an azotated matter analogous to fibrine, with which at a later period are associated the cellular tissue, the ligneous tissue, and the amylaceous tissue itself. This azotated matter, the real origin of all the parts of the plant, is never destroyed; it is always to be found, however abundant may be the non-azotated matter which has been interposed between its particles.

This azote, fixed by plants, serves therefore to produce a concrete fibrinous substance which constitutes the rudiment of all the organs of the vegetable.

It also serves to produce the liquid albumen which the coagulable juices of all plants contain, and the caseum, so often confounded with albumen, but so easy to recognize in many plants.

Fibrin, albumen and caseum, exist then in plants. These three products, identical in their composition, as M. Vogel has long since proved, offer a singular analogy with the ligneous matters, the amidon, and the dextrine.

Indeed, fibrin is like ligneous matter, insoluble; albumen, like starch, coagulates by heat; caseum, like dextrine, is soluble.

These azotated matters moreover are neutral, as well as the three parallel non-azotated matters; and we shall see that by their abundance in the animal kingdom they act the same part that these latter exhibited to us in the vegetable kingdom.

Besides, in like manner as it suffices for the formation of non-azotated neutral matters, to unite carbon with water or with

its elements, so also for the formation of these azotated neutral matters, it suffices to unite carbon and ammonium with the elements of water; forty-eight molecules of carbon, six of ammonium, and seventeen of water, constitute, or may constitute, fibrine, albumen and caseum.

Thus in both cases, reduced bodies, carbon or ammonium, and water, suffice for the formation of the matters which we are considering, and their production enters quite naturally into the circle of reactions, which vegetable nature seems especially adapted to produce.

The function of azote in plants is therefore worthy of the most serious attention, since it is this which serves to form the fibrin which is found as the rudiment in all the organs, since it is this which serves for the production of the albumen and caseum, so largely diffused in so many plants, and which animals assimilate or modify according to the exigencies of their own nature.

It is in plants then that the true laboratory of organic chemistry resides;—thus carbon, hydrogen, ammonium and water, are the principles which plants elaborate; ligneous matter, starch, gums, and sugars on the one part, fibrin, albumen, caseum and gluten on the other, are then the fundamental products of the two kingdoms; products formed in plants and in plants alone, and transferred by digestion into animals.

Ashes.—An immense quantity of water passes through the vegetable during the period of its existence. This water evaporates at the surface of the leaves and necessarily leaves as residue, in the plant, the salts which it contained in solution. These salts compose the ashes, products evidently borrowed from the earth, to which, after their death, vegetables give it back again.

As to the form in which these mineral products deposit themselves in the vegetable tissue, nothing can be more variable. We may remark, however, that among the products of this nature, one of the most frequent and most abundant is that pectinate of lime, discovered by M. Jacquelain in the ligneous tissue of most plants.

IV.—If, in the dark, plants act as simple filters which water and gases pass through; if, under the influence of solar light they act as reducing apparatus which decompose water, carbonic acid and oxide of ammonium, there are certain epochs and certain organs in which the plant assumes another, and altogether opposite part.

Thus, if an embryo is to be made to germinate, a bud to be unfolded, a flower to be fecundated, the plant which absorbed

the solar heat, which decomposed carbonic acid and water, all at once changes its course. It burns carbon and hydrogen; it produces heat, that is to say, it takes to itself the principal characters of animal life.

But here a remarkable circumstance reveals itself. If barley or wheat is made to germinate, much heat, carbonic acid and water are produced. The starch of these grains first changes into gum, then into sugar, then it disappears in producing carbonic acid, which the germ is to assimilate. Does a potato germinate, here also it is its starch which changes into dextrine, then into sugar, and which at last produces carbonic acid and heat. Sugar, therefore, seems the agent by means of which plants develop heat as they need it.

How is it possible not to be struck from this with the coincidence of the following facts?—Fecundation is always accompanied by heat; flowers as they breathe produce carbonic acid. They therefore consume carbon; and if we ask whence this carbon comes, we see in the sugar cane, for example, that the sugar accumulated in the stalk has entirely disappeared when the flowering and fructification are accomplished. In the beet root, the sugar continues increasing in the roots until it flowers; the seed-bearing beet contains no trace of sugar in its root. In the parsnep, the turnip and the carrot, the same phænomena take place.

Thus at certain epochs, in certain organs, the plant turns into an animal; it becomes like it an apparatus of combustion; it burns carbon and hydrogen; it gives out heat.

But at these same periods, it destroys in abundance the saccharine matters which it had slowly accumulated and stored up. Sugar, or starch turned into sugar, are then the primary substances by means of which plants develop heat as required for the accomplishment of some of their functions.

And if we remark with what instinct animals, and men too, choose for their food just that part of the vegetable in which it has accumulated the sugar and starch which serve it to develop heat, is it not probable, that, in the animal œconomy, sugar and starch are also destined to act the same part, that is to say, to be burned for the purpose of developing the heat which accompanies the phænomenon of respiration?

To sum up, as long as the vegetable preserves its most habitual character, it draws from the sun heat, light, and chemical rays. From the air it receives carbon, from water it takes hydrogen, azote from the oxide of ammonium, and different salts from the earth. With these mineral or elementary substances, it composes the organized substances which accumulate in its tissues.

They are ternary substances, ligneous matter, starch, gums and sugars.

They are quaternary substances, fibrin, albumen, caseum, and gluten.

So far then the vegetable is an unceasing producer; but if at times, if to satisfy certain wants, the vegetable becomes a consumer, it realizes exactly the same phænomena which the animal will now set before us.

V.—An animal in fact constitutes an apparatus of combustion from which carbonic acid is continually disengaged, in which consequently carbon undergoes combustion.

You know that we were not stopped by the expression *cold-blooded animals*, which would seem to designate some animals destitute of the property of producing heat. Iron, which burns vividly in oxygen, produces a heat which no one would deny; but reflection and some science is necessary in order to perceive, that iron which rusts slowly in the air disengages quite as much, although its temperature does not sensibly vary. No one doubts that lighted phosphorus in burning produces a great quantity of heat. Unkindled phosphorus also burns in the air, and yet the heat which it develops in this state was for a long time disputed.

So as to animals, those which are called warm-blooded burn much carbon in a given time, and preserve a sensible excess of heat above the surrounding bodies; those which are termed cold-blooded burn much less carbon, and consequently retain so slight an excess of heat, that it becomes difficult or impossible to observe it.

But nevertheless, reflection shows us that the most constant character of animal existence resides in this combustion of carbon, and in the development of carbonic acid which is the result of it, beginning also in the production of heat which every combustion of carbon occasions.

Whether the question be of superior or inferior animals; whether this carbonic acid be exhaled from the lungs or from the skin, does not signify; it is always the same phænomenon, the same function.

At the same time that animals burn carbon, they also burn hydrogen; this is a point proved by the constant disappearance of hydrogen which takes place in their respiration.

Besides, they continually exhale azote. I insist upon this point, and principally in order to banish an illusion which I cannot but believe to be one of the most prejudicial to your studies. Some observers have admitted that there is an absorption of azote in respiration, but which never appears un-

accompanied by circumstances that render it more than doubtful. The constant phænomenon is the exhalation of gas.

We must therefore conclude with certainty, that we never borrow azote from the air; that the air is never an aliment to us; and that we merely take from it the oxygen necessary to form carbonic acid with our carbon, and water with our hydrogen.

The azote exhaled proceeds then from the aliments, and it originates from them entirely. This, in the general œconomy of nature, may in thousands of centuries be absorbed by plants, which, like Jerusalem artichokes, draw their azote directly from the air.

But this is not all the azote which animals exhale. Every one gives out by the urine, on an average, as M. Lecanu has proved, 230 grains of azote a day, of azote evidently drawn from our food, like the carbon and hydrogen which are oxidized within us (*que nous brûlons*).

In what form does this azote escape? In the form of ammonia. Here indeed, one of those observations presents itself which never fail to fill us with admiration for the simplicity of the means which nature puts in operation.

If in the general order of things we return to the air the azote which certain vegetables may sometimes directly make use of, it ought to happen that we should also be bound to return ammonia, a product so necessary to the existence and development of most vegetables.

Such is the principal result of the urinary secretion. It is an emission of ammonia, which returns to the soil or to the air.

But is there any need to remark here, that the urinary organs would be changed in their functions and in their vitality by the contact of ammonia? the contact of the carbonate of ammonia would even effect this; and so nature causes us to excrete urea.

Urea is carbonate of ammonia, that is to say, carbonic acid like that which we expire, and ammonia such as plants require. But this carbonate of ammonia has lost of hydrogen and oxygen, so much as is wanting to constitute two molecules of water.

Deprived of this water the carbonate of ammonia becomes urea; then it is neutral, not acting upon the animal membranes; then it may pass through the kidneys, the ureters, and the bladder, without inflaming them; but having reached the air, it undergoes a true fermentation, which restores to it these two molecules of water, and which makes of this same urea true carbonate of ammonia; volatile, capable of ex-

haling in the air; soluble, so that it may be taken up again by rain; and consequently destined thus to travel from the earth to the air and from the air to the earth, until, pumped up by the roots of a plant and elaborated by it, it is converted anew into an organic matter.

Let us add another feature to this picture. In the urine, along with urea, nature has placed some traces of albuminous or mucous animal matter, traces which are barely sensible to analysis. This, however, when it has reached the air, is there modified, and becomes one of those ferments of which we find so many in organic nature; it is this which determines the conversion of urea into carbonate of ammonia.

These ferments, which have so powerfully attracted our attention, and which preside over the most remarkable metamorphoses of organic chemistry, I reserve for the next year, when I shall give you a still more particular and full account of them.

Thus we discharge urea accompanied by this ferment, by this artifice, which acting at a given moment, turns this urea into carbonate of ammonia.

If we restore to the general phænomenon of animal combustion that carbonic acid of the carbonate of ammonia which of right belongs to it, there remains ammonia as the characteristic product of urine.

Thus, by the lungs and the skin, carbonic acid, water, azote;

By the urine, ammonia.

Such are the constant and necessary products which exhale from the animal.

These are precisely those which vegetation demands and makes use of, just as the vegetable in its turn gives back to the air the oxygen which the animal has consumed.

Whence come this carbon, this hydrogen burnt by the animal, this azote which it has exhaled in a free state or converted into ammonia? They evidently come from the aliments.

By studying digestion in this point of view, we have been led to consider it in a manner much more simple than is customary, and which may be summed up in a few words.

In fact, as soon as it was proved to us that the animal creates no organic matter; that it merely assimilates or expends it by burning it (*en la brûlant*), there was no occasion to seek in digestion all those mysteries which we were quite sure of not finding there.

Thus digestion is indeed but a simple function of absorption. The soluble matters pass into the blood, for the most part unchanged; the insoluble matters reach the chyle,

sufficiently divided to be taken up by the orifices of the chyliferous vessels.

Besides, the evident object of digestion is to restore to the blood a matter proper for supplying our respiration with the ten or fifteen grains of coal, or the equivalent of hydrogen which each of us burns every hour, and to restore to it the grain of azote which is also hourly exhaled, as well by the lungs or the skin as by the urine.

Thus the amylaceous matters are changed into gum and sugar; the saccharine matters are absorbed.

The fatty matters are divided, and converted into an emulsion, and thus pass into the vessels, in order to form depôts which the blood takes back and burns as it needs.

The neutral azotated substances, fibrin, albumen and caseum, which are at first dissolved, and then precipitated, pass into the chyle greatly divided or dissolved anew.

The animal thus receives and assimilates almost unaltered the azotated neutral substances which it finds ready formed in the animals or plants upon which it feeds; it receives fatty matters which come from the same sources; it receives amylaceous or saccharine matters which are in the same predicament.

These three great orders of matters, whose origin always ascends to the plant, become divided into products capable of being assimilated, fibrin, albumen, caseum, fatty bodies, which serve to renew or recruit the organs with the combustible products, sugar and fatty bodies which respiration consumes.

The animal therefore assimilates or destroys organic matters ready formed; it does not create them.

Digestion introduces into the blood organic matters ready formed; assimilation employs those which are azotated; respiration burns the others.

If animals do not possess any peculiar power for producing organic matters, have they at least that special and singular power which has been attributed to them of producing heat without expenditure of matter?

You have seen, while discussing the experiments of MM. Dulong and Despretz, you have positively seen the contrary result from them. These skilful physicists supposed that an animal placed in a cold water calorimeter comes out of it with the same temperature that it had on entering it; a thing absolutely impossible, as is now well known. It is this cooling of the animal, of which they took no account, that expresses in their *tableaux* the excess of heat attributed by them and by all physiologists to a calorific power peculiar to the animal and independent of respiration.

It is evident to me that all animal heat arises from respiration; that it is measured by the carbon and hydrogen burnt. In a word, it is evident to me that the poetical comparison of a rail-road locomotive to an animal is founded on a more serious basis than has perhaps been supposed. In each there are combustion, heat, motion, three phænomena connected and proportional.

You see, that thus considering it, the animal machine becomes much easier to understand; it is the intermediary between the vegetable kingdom and the air; it borrows all its aliments from the one, in order to give all its excretions to the other.

Shall I remind you how we viewed respiration, a phænomenon more complex than Laplace and Lavoisier had thought, or even Lagrange had supposed, but which precisely, as it becomes complicated, tends more and more to enter into the general laws of inanimate nature?

You have seen that the venous blood dissolves oxygen and disengages carbonic acid; that it becomes arterial without producing a trace of heat. It is not then in becoming arterial that the blood produces heat.

But under the influence of the oxygen absorbed, the soluble matters of the blood change into lactic acid, as MM. Mitscherlich, Boutron-Charlard and Fremy observed; the lactic acid is itself converted into lactate of soda; this latter by a real combustion into carbonate of soda, which a fresh portion of lactic acid decomposes in its turn. This slow and continued succession of phænomena which constitutes a real combustion, but decomposed at several times, in which we see one of the slow combustions to which M. Chevreul drew attention long ago, this is the true phænomenon of respiration. The blood then becomes oxygenized in the lungs; it really breathes in the capillaries of all the other organs, there where the combustion of carbon and the production of heat principally take place.

A last reflection. To ascend to the summit of Mont-Blanc, a man takes two days of twelve hours. During this time, he burns at an average 300 grammes of carbon, or the equivalent of hydrogen. If a steam-engine had been employed to take him there, it would have burnt from 1000 to 1200 to accomplish the same work.

Thus, viewed as a machine, borrowing all its power from the coal that it burns, man is an engine three or four times more perfect than the most perfect steam-engine. Our engineers have therefore still much to do, and yet these numbers are quite such as to prove that there is a community of

principles between the living engine and the other; for if we allow for all the inevitable losses in steam-engines which are so carefully avoided in the human machine, the identity of the principle of their respective powers appears manifest and clear.

But we have followed far enough considerations as to which your own reflections are already in advance of me, and where your recollections leave me nothing more to do.

To sum up, then, we see that of the primitive atmosphere of the earth three great parts have been formed:

One which constitutes the actual atmospheric air; the second, which is represented by vegetables, the third by animals.

Between these three masses, continual exchanges take place: matter descends from the air into plants, enters by this route into animals, and returns to the air according as these make use of it.

Green vegetables constitute the great laboratory of organic chemistry. It is they which, with carbon, hydrogen, azote, water and oxide of ammonium, slowly build up all the most complex organic matters.

They receive from the solar rays, under the form of heat or of chemical rays, the powers necessary for this work.

Animals assimilate or absorb the organic matters formed by plants. They change them by little and little, they destroy them. In their organs, new organic substances may come into existence, but they are always substances more simple, more akin to the elementary state than those which they have received. By degrees these decompose the organic matters slowly created by plants; they bring them back little by little towards the state of carbonic acid, water, azote and ammonia, a state which allows them to be returned to the air.

In burning or destroying these organic matters, animals always produce heat, which radiating from their bodies in space, goes to supply the place of that which vegetables had absorbed.

Thus all that air gives to plants, plants give up to animals, and animals restore it to the air, an eternal circle in which life keeps in motion and manifests itself, but in which matter merely changes place.

The brute matter of air, organized by slow degrees in plants, comes, then, to perform its part without change in animals, and serves as an instrument for thought; then vanquished by this effort and broken, as it were, it returns brute matter to the great reservoir whence it came.

Allow me to add, in finishing this picture, which sums up

opinions which to my mind are but the necessary consequences and developments of the great path which Lavoisier marked out for modern chemistry ; allow me, I say, to express myself as he did with regard to his fellow-labourers and his friends.

If in my lessons, if in this summing up, I have chanced to adopt without mentioning them the experiments or the opinions of M. Boussingault, it is that the habit of communicating to each other our ideas, our observations, our manner of viewing things, has established between us a community of opinions, in which we ourselves even afterwards find it difficult to distinguish what belongs to each of us.

In resting these opinions and their consequences on his name and on his authority, in telling you that we work actively, sometimes together, and sometimes apart, in order to verify and to develop all these facts, all these results by experiment, I do but evince my anxious desire to justify the interest which you have this year taken in my labours.

For this I beg to thank you. It has given me courage to undertake a long course of researches; if anything useful to the progress of humanity should result from them, let all the honour of it redound to the intelligent good will with which you have constantly surrounded me, and for which I shall ever be profoundly grateful.

LXX. *On the question, Whether there are any evidences of the former existence of Glaciers in North Wales?* *By* J. E. Bowman, *F.L.S. & F.G.S.* *

THE recent discovery of traces of ancient glaciers in Scotland and the north of England, rendered it probable that similar appearances would be found in North Wales, where, though situated in a somewhat lower latitude, the slight increase of temperature is fully counteracted by the greater altitude of the central mountain group. The geographical situation of all these parts of our island with respect to the sea, and the geological periods of their elevation may be considered to be so nearly the same, that if glaciers can be satisfactorily proved to have once existed in one, there seems no reason to doubt their occurrence in the rest, even admitting them to have had a much more local origin and limited range than Prof. Agassiz has supposed. If we compare the mountains of Britain with those of Switzerland, where the most un-

* Communicated by the Author.

doubted evidence of the former greater extent of glaciers may be gradually traced from the warm valleys of the Alps up to the actual feet of existing glaciers, the greater altitudes of the mountains and increased distance from the modifying influence of the sea, will be found more than adequate to compensate for their being situated ten degrees nearer to the equator. In this view it is therefore possible, assuming the theory of Prof. Agassiz of a general reduction of temperature in ancient times over the whole northern hemisphere, that Switzerland might be covered with ice and snow, while Britain might be comparatively free from them. But this reasoning, though plausible on a cursory glance, falls to the ground, if we accept the theory of this distinguished philosopher, that the glacial period occurred antecedent to the elevation of the Alps. There is abundance of direct geological evidence to prove that they have been raised, at least much higher than before, since the deposition and consolidation of the tertiary strata; and as it is scarcely less certain that the primary mountains of Britain were raised at a much earlier period, it seems to follow from their altitude and higher latitude, that the surrounding region must have been the seat of more intense cold than Switzerland, *then* without high mountains and situated in a much warmer climate.

Having spent a portion of the late summer in endeavouring to trace the geological sequence of the schistose rocks of North Wales, I have been naturally led into some high and unfrequented tracts where it seemed probable, if anywhere, that evidence of glaciers would be found. I have examined many of the main and lateral valleys on the east and south flanks of the Snowdon group, of the Arenigs, and of the north end of Berwyn chain, as well as of the moorland hills of Denbighshire, between Cerrig y Druidion and Llanrwst; also those which intersect the wild and barren hills that occupy the country between the great Holyhead road and Bala, and form the dividing ridge between Merionethshire and Denbighshire. These valleys descend from the highest mountains of North Wales, and are the troughs through which the numerous tributaries of the Dee, the Clwyd, and the Conway, are carried into the lower parts of the country. Many of them, from their smooth uniform slopes and easy inclinations, seem calculated to give rise to, and to retain those appearances which glaciers usually create and leave behind them. In none of them, however, have I found any features which appeared to me to have been produced by glaciers; none at least have retained that outward form or shape which seemed to justify me in referring them to such an origin.

There are, however, various appearances scattered here and there through the districts I have named, which, if they have not originated in glaciers, bear a sufficient resemblance to require a close examination. It is more important to make this examination than may at first sight appear, in order to guard those who have not witnessed the actual effects of glacial action from attributing every accumulation of gravel or streaked rock to such an origin. Though satisfied myself, from personal observation, that existing glaciers form moraines on their sides and at their feet, and abrade the rocks over which they move, so as to scratch and polish their surfaces, it has constantly been my practice to see, in the first place, if the analogous appearances I have met with in North Wales were not capable, on taking into account all the local circumstances, of satisfactory explanation by some obvious existing cause; conceiving it to be improper hastily to refer to the operation of ice, effects which would admit of an equally easy explanation from the transporting or erosive power of water.

Of true lateral moraines ranging along the slopes of the valleys more or less horizontally, and formed either of stones or gravel, no instance has fallen under my observation. I passed through many valleys, where, from the gradual rise of the sides and the laminar or schistose structure of the rock, the detritus from above had lodged and accumulated into a talus, or formed continuous inclined planes of great uniformity, which were often covered with herbage. This was strikingly seen in the valley above Penmachno, but I could not discover any trace of lateral moraines, nor of terminal ones, about the outlet of the valley into the great trough of the Conway. In the neighbouring valley of the infant Conway, between Yspytty and Pentre Voelas, where the river first runs parallel with the great Holyhead road, the slopes are covered on both sides by a succession of rounded wavy knolls or swellings, which are very striking in such a barren and rugged district, from their smoothness and rich verdure, a character by which they may be recognised at a considerable distance. Near the lower end of the valley, on the east side, several of them are detached, and are about fifty yards diameter and twenty or thirty yards high; but they usually run into one another and are of no regular shape, except that they all slope towards the stream, so that they look like great undulating and projecting breastworks and bastions thrown up for military defence. Some of these mounds cover an area of 100 acres, and are two or three hundred feet high, looking like little hills, but without any regularity or uniformity in the grouping, their bases running into one another and sloping towards

the middle of the valley on both sides. One only near the little stream of the Clettwr and above the road, seemed, when seen from a distance, to have a pretty level top; but I observed none with steep sides or escarpments, as though they had been eroded or cut down by a current. The road from Pentre Voelas to Yspytty winds among them, the higher ones being above it to the S.E., the rest inclining towards the valley below. Similar mounds mask the rocky slope on the opposite bank of the Conway as high as half a mile above Yspytty, becoming gradually lower and thinner till they disappear altogether. In a north-west direction they extend from Pentre Voelas to the opening of the valley of the Machno, and may often be seen on both sides the great road in the less precipitous parts of the gorge, though more frequently and more distinctly on the left bank of the river. From partial openings into several of them, I saw they were formed of local drift or gravel from the neighbouring dark schistose rocks, without any large boulders. In one of them, near the great road, there was a streaked or bedded character, the layers being formed of pebbles of different sizes, as though they had been left by a current which had varied in intensity. It appeared to me that these hills were not composed throughout of this gravelly drift, but that this material only formed a coat or covering upon the previously existing prominences of the lower Silurian schist of the district, sufficiently thick to conceal the rocky nucleus and to give it a smooth rounded outline. Neither could I attribute their shape to any form or modification of lateral moraine; and I was of opinion, that even should any one contend that they had originated in a glacier, it would be necessary to admit they must have been subsequently modified by water. It is not easy to understand how so large a mass of diluvium could have been accumulated in this particular locality. Seeing that the river Conway, soon after passing through it, has to make its way along a narrow gorge, and that from the precipitous form of the rocks on each side, it must always have flowed in the same channel, I was at first inclined to suppose that some obstruction might have filled this gorge and pounded up the river and the detritus it had brought down from the higher lands. But on examination of their relative levels, it appeared that the gravelly hills above the road east of Yspytty were higher than the surface of a lake so dammed up, and consequently they could not have originated from such a cause. It is singular that in no other neighbouring valley of similar character, were there any accumulations of the same material, and further observation will be necessary to explain their origin.

The next class of appearances I have met with in North Wales resembling those produced by glacier action, are the striæ or scratches on the surfaces of rocks. Of these I have found several decided and strongly marked instances; and as they are due to various causes, and can in some cases be demonstrated *not* to have been connected with glaciers, it may be useful to describe them somewhat in detail.

The first I met with occurs in the Upper Silurian rocks on the right bank of the Dee, in the immediate neighbourhood of Llangollen. Here the true stratification is easily ascertained, since it is not affected by any cleavage, though the rock has a system of joints which divide the beds diagonally to the planes of the surface into irregular rhombs. The beds are nearly horizontal, having an average dip to the S.S.E. of about five or six degrees; and they rise to the height of from 300 to 400 feet, without any apparent dislocation or change in the direction of the dip. At the base of this precipitous hill, and in a little quarry, the outcrop of the beds is seen in various places; their surfaces are generally smooth and uniformly plane, occasionally slightly undulated. I observed the level surface of one of them to be streaked or traced with strong and perfectly parallel grooves or furrows, some as fine as though they were made by the point of a pin, and others the eighth of an inch wide, promiscuously and closely ranged alongside others of intermediate dimensions. The whole surface was occupied by these grooves and their intermediate ridges, and had the appearance of having been planed with a tool with a snipped or indented edge. Some of the broader furrows were truncated and terminated abruptly, and one or more narrower ones seemed to be connected with and to issue from them. The range or bearing of the striæ was N.N.W. and S.S.E., and the breadth of surface exposed was from twenty-five to thirty yards, every part of which was channeled in the manner described. It was at once evident, from the general appearance of the quarry, that this bed had never been exposed or disturbed from those above or below it, since it was closely and conformably overlaid by the mass above, the whole having a slight inclination to the S.S.E. The furrows were also evidently continued inwards upon the yet undisturbed portions; but to remove all doubt upon this head, I employed a quarryman to cut away the superincumbent bed more than a foot inwards from the face, where they were found to be equally clear and strong. The specimens I obtained were from this part, and had never before been exposed since the original deposition of the beds. The under surface of the superincumbent bed was moreover similarly though more faintly streaked;

in some places so obscurely, that it would not have been perceived to be so on casual inspection; but in others, very clearly. There was another bed lying about a foot lower in the series, similarly though less distinctly striated; and the striæ had exactly the same direction.

It was therefore abundantly clear that these grooved striæ could not have been produced by glacial action, or indeed by any cause operating after the original deposition or consolidation of the beds; inasmuch as they are nearly at *right angles* to the course of the river, and therefore to the *direction* of a glacier, and especially as they are found under beds in close contact, that have never suffered any partial or relative disturbance *inter se**. Since, however, they exist on two surfaces in contact, and somewhat resemble those polished portions of rocks well known by the name of Slickensides, which are believed to be the effect of friction on two surfaces rubbed against each other, it is worth while to examine whether they might not have been produced by a similar action. As already stated, the appearance of the quarry and precipice above the road shows that the rock has been tilted aside *en masse*, and has not been affected by any partial or subsequent dislocation; and the nearly horizontal position of the beds renders it extremely improbable, if not impossible, that they could have been so rubbed against each other as to produce the furrows in question. The appearance called Slickensides is found on the perpendicular or slanting sides of fissures and veins, and is caused by the unequal and repeated motion of the two surfaces against each other. But in proportion as the direction of these sides varies from the slanting to the horizontal position, this kind of action diminishes, from the increasing difficulty of maintaining motion on the horizontal plane. Great lateral pressure, from the gravity of an upheaved mass, may suddenly and for once act horizontally; but a succession of alternating throes or paroxysms can only be propagated from below; from any other point, motion would soon be overcome by the *vis inertiæ* of the mass. It is easy to understand how the upper portion of a stratified rock may slide off its base when but gently inclined; but it is scarcely credible that any possible circumstances could cause it, in such a position, to be moved backwards and forwards long enough to produce the grooved appearance impressed upon the bed in question.

* It is scarcely necessary to say that they differ essentially from the furrows which traverse the mountains of Scandinavia, as described by Seffström (see the last Part of Taylor's Scientific Memoirs); and from the furrowed and polished rocks of Fontainbleau, as noticed by Durocher in the Phil. Mag. for Nov. 1841.

Again, an examination of the striæ in hand specimens will show that some of the more delicate lines diverge from the general parallelism, and unite with the contiguous ones without suffering interruption from the intervening furrows. In some parts of the surface there are groups of faint transverse elevations and depressions which run across many contiguous striæ with a gentle curve; and in others the grooving is not confined to a single plane, but seems to penetrate beneath the surface, and to pervade several laminæ inwards. None of these appearances could have been produced by a hard body sliding along its surface in right lines, as in slickensides; and I regret my inability to offer any satisfactory explanation. The only conjecture I can make is, whether, as these striæ lie in the direction of the magnetic meridian, they may not have been produced by some electric or magnetic power acting upon the chemical ingredients of the bed or upon its surface, and causing them to assume a polaric direction while yet in a soft or semi-indurated state. They belong to a class of phænomena hitherto little noticed and still less understood, but which are nevertheless of great interest and worthy of especial attention, from the important bearing they have on questions of physical geology, about which we are at present almost totally in the dark, but which may one day throw light on the original structure of our globe.

I come now to describe a still more curious case of striated surface, found on a system of rocks lying geologically far below that just mentioned, and which, though unfossiliferous, will range with the Lower Silurian formation. It occurs in the bed of a little mountain stream named Clettwr, that falls into the Conway a mile or two below Yspytty, having cut its way through the great mounds of gravel already described in that vicinity and exposed the solid rock beneath. The rock is a very hard blue schist, in thick compact beds, with a dip of 15° N.N.E. and an obscure cleavage dipping 48° N.E., which has but partially obliterated the bedding. The true stratification is indicated by many thin streaked laminæ which appear as lines in the section, and are quite parallel with the undulated surface of the bed about to be described. This surface is somewhat glossy and very strongly marked with a system of wavy lines that preserve a general parallelism, but run into one another, and do not long preserve their individual and separate character. The surface may be more correctly said to be corrugated and undulated than grooved, for the striated lines do not possess the perfect parallelism and continuity of those near Llangollen, though they maintain an uniformity of character, are persistent over every part of the surface that was exposed,

and were also found at intervals of a few yards, wherever I removed the overlying strata. Their direction is E.S.E. and W.N.W., or 45° W. of the magnetic meridian, and at right angles with the direction of the valley. But the most singular feature of this bed is a second system of closer, finer, and more regular lines which cross the larger undulations at an angle of about 70°, traversing their intermediate hollows as well as their crests and sides, thus covering the whole surface, and giving it a reticulated appearance very difficult to describe. Now and then, at irregular intervals, there is an occasional thicker line; or perhaps it would be more correct to say that the interval between the fine lines is greater in these places than usual. This double system of lines seems to originate in a thin plate of somewhat different material, about the eighth of an inch thick, and interstratified with the schist; for the lines cover both its upper and under surface, and the still thinner laminæ of which this plate is composed, are seen by their darker and lighter shades, to be undulated also throughout its whole thickness. This plate easily separates from the contiguous thicker beds, and when removed, is found to have left a sharp and perfect impression of both sets of lines on each of the surfaces with which it was in contact. Not only so, but the wavy lines have affected and penetrated the thicker beds, rendering them more glossy, and giving to the split of the cleavage a corresponding undulation which gradually dies away at the depth of one-third or half an inch beneath the surface. This is the more singular, as the cleavage planes cut the surface at an angle of 15° with the corrugated lines, and throw the undulations out of their regular course. There is another somewhat similar marking on the surface of a second bed in the same brook, about a foot higher in the series, and in the same direction or line of strike. Here the lines are broader and have been deeper; but being in the direct course of the stream, they are much obliterated by the water passing over them, and I did not bring away a specimen.

The reasoning already employed to show that the striæ on the Upper Silurian bed near Llangollen could not have been formed by a glacier, nor by slickensides action, is in a great measure applicable to those now under consideration; it applies to them, if possible, with greater force, since the double series of intersecting lines offers the most decisive evidence that they could not have been produced by either. In speculating as to their real origin, I will only add to the suggestion before thrown out, that when this class of phænomena shall be better understood, the specimen in the bed of the Clettwr may possibly throw some light on the obscure subject of cleavage.

A slight notice of a different kind of striæ on the Welsh rocks, arising from another cause and capable of easy explanation, will be sufficient. At the north end of Trefriw, in the vale of Llanrwst, by the side of the road to Conway, large surfaces of the true beds of Lower Silurian rocks are laid bare in several different places; they have an average dip of about 18° to the E.S.E., and form smooth and regular inclined planes. This arises from wayboards of softer shale, or from slight adhesion of the beds. On one of these inclined planes to the north of the little quay, are many nearly parallel lines or striæ, running in the direction of the valley, which are very liable to be mistaken for those produced by glaciers grating upon their bed. But on close examination I found them to be only the ends or sections of one of the series of joints which pervaded the bed and divided it nearly at right angles to its surface. The very general occurrence of joints symmetrically arranged in the Silurian rocks, makes it necessary to guard against deception arising from this cause. I will only add, under this head, a short notice of one or two patches of rocks by the side of the Holyhead road, where it overlooks the great chasm opening into Glyn Lledr, a mile east of the Waterloo Bridge, which have the appearance of being somewhat striated and polished. But the surfaces are also irregularly undulated, and the dip is to the south, or diagonal to the direction of the trough of the Conway; and there is a very steep hill immediately above them, down which, if these striæ were produced by glaciers, the icy mass must have descended, a supposition which inspection shows to be impossible. Besides, the slopes of the great opening from Glyn Lledr are very favourable for the formation and preservation of lateral moraines, of which I could observe no traces. And it may be remarked generally, that as both lateral and terminal moraines are more frequently formed on the outskirts of glaciers than striæ on the rocks beneath, and are from their situation and the nature of the material, more likely to be preserved as monuments to after ages, their occurrence in North Wales, either accompanied by striated rocks, or alone in other parts of the country, would have added to the probability of their common origin; while their entire absence from localities so favourable for their production, makes it necessary to exercise the greater caution, in referring to glacial action the other phænomena described in this paper.

I met with but one example of rock with a rounded or domed surface, to which Saussure has given the name of "*roches moutonnées*," and this was in a locality where it seemed so much more natural to refer it to a cause still in operation,

that it was not necessary to call in the aid of glaciers. The spot is well known to tourists as one of the wildest and most alpine in North Wales, where the torrents from Llyn Ogwen and Llyn Idwal, on issuing from their respective lakes, are precipitated over the rocky barrier that forms the head of Nant Francon. Standing on the bridge between these two torrents, the rocks on both sides, that form, as it were, the crest of this barrier, and obstruct the free passage of the water, seem to have a smooth or rounded form that is evidently not natural. That in the stream of the Ogwen is the largest and the most regularly domed; and the side next the bridge having been recently cut away in the direction of the cleavage, their planes form a perpendicular face, on which the true lines of stratification may be traced with sufficient distinctness. The dip is towards the S.E. at an angle of about 40°, thus proving the rounded top to have no connexion with either of the divisional planes just named, or with the intersecting joints, which, as usual, have flat surfaces. Though rounded, the surface is not polished, but has a dull and weathered appearance, the effect of abrasion rather than of friction, on the hard hornstone porphyry, of which the rock is composed.* These rounded bosses of rock are at present some feet higher than the level of the respective torrents that wash their bases; but it is not difficult to believe that a period has existed when they were covered by the torrent, and that in the lapse of ages, the stones and ice it has carried along with it, having given them this rounded form, have continually deepened the channel, till the domed surfaces have been left projecting in their present situation.

Such are the principal appearances I have observed in North Wales, which approximate to the effects produced by existing glaciers upon the rocks and debris by which they are surrounded. The phænomena of moraines interested me much in Switzerland; and the recollections of their singular features would, I think, have enabled me to detect any traces of similar remains in the mountainous district under consideration. I would not, however, be understood as meaning to deny their existence in it altogether; for there are still so many obscure valleys that I have not visited, at least since the discovery of the remains of glaciers in Britain, that it would be presumptuous to speak dogmatically upon so obscure a subject. My object has been to confine myself to what I have

* Several varieties of hornstone porphyry are seen near this spot; and on the ascent to Cwm Idwal a thick perpendicular vein of hornstone schist is exposed, and is thrown by a dislocation on the opposite side of the road west of the bridge. It is extracted for whetstones.

seen during the late summer, to describe appearances with accuracy, and refer them to their real causes, independently of any theory. Believing that glaciers once existed in Scotland and in the north of England, I thought it probable traces of them might be found in Wales also; and I have shown that not a few appearances there may, on a hasty survey, be referred to such an origin. But believing, after deliberate examination, that these have either been produced by other adequate causes, or *could not* have been due to glacier action, I have felt myself bound honestly to state the conclusions I have arrived at, being satisfied from some experience, that to allow the observation or the judgement to be warped by preconceived theory, however plausible, or to decide on partial insufficient evidence, must be ultimately injurious to the cause of truth.

Manchester, Oct. 18, 1841.

LXXI. *Calculation of Logarithms by means of Algebraic Fractions.* *By the Rev.* R. Murphy, *M.A.**

THE logarithms here used are Napierian, which may be readily converted into logarithms of any other system, simply multiplying them by the modulus.

Take the coefficients of a binomial raised to any positive and integer power n, and the same number of the coefficients of a binomial raised to the next superior negative power, $-(n+1)$; multiply both sets, term by term, multiply the products by the successive powers of a number as $1, t, t^2, \dots t^n$, and connecting all the terms thus formed by the sign $+$, this sum is the denominator of the fraction.

Multiply the last term but one of this denominator by $\frac{2}{n}$, the last but two by $\frac{2}{n}+\frac{2}{n-1}$, the last but three by $\frac{2}{n}+\frac{2}{n-1}+\frac{2}{n-2}$, and so on; connect the products with the sign $+$, the sum thus obtained is the numerator of the fraction.

The fraction thus formed is to be added to $\log t$ in order to obtain $\log(t+1)$.

The sum is always deficient.

Call the above denominator P_n and put $T = 4t(t+1)$, the

* Communicated by the Author.

deficiency is greater than $\frac{1.2.3\ldots n}{1.3.5\ldots(2n+1)} \cdot \frac{2}{P_n} \cdot \frac{1}{(T+1)^{\frac{n+1}{2}}}$,

and less than $\frac{1.2.3\ldots n}{1.3.5\ldots(2n+1)} \cdot \frac{2}{P_n} \cdot \frac{1}{T^{\frac{n+1}{2}}}$.

The number n is arbitrary, but had better be taken large when the number t is small.

If we take $n = 10$ when $t = 1$, we find by this rule log 2 $= \frac{5612726\frac{43}{60}}{8097453}$, which converted to a decimal is necessarily accurate to the 15th place inclusive, which is known by the above limits.

By the application of this and similar methods, a mere arithmetician will be able to calculate logarithms, accurate to any extent, and to their last decimal figure.

Robert Murphy.

P.S. In my paper contained in the last Number, p. 369, for "*a* is a primitive radix," read *a* is the next superior primitive radix. The following numbers are verified instances of the general formula in that paper, viz. $(3n^2+2)$5, 29, 149; $(5n^2+3)$ 23, 83; $(15n^2+13)$, 73; $(20n^2+17)$ 37; $(21n^2+19)$, 103, and several others, and I have not met with an exceptional case.

LXXII. *On the Action of Metallic Copper on Solutions of certain Metals, particularly with reference to the detection of Arsenic.* *By* H. Reinsch*.

M. REINSCH was induced to enter into the examination of the action of copper on certain metallic solutions, and its application as a test for arsenic, from the circumstance of having obtained a precipitate of metallic arsenic upon a piece of copper immersed in commercial hydrochloric acid.

This action of metallic copper he considered the more important, as by that means he hoped to be able to devise a simpler method of detecting arsenic, and also of separating it from solutions, which might be less liable to objection than that by Marsh's apparatus, the uncertainty of which has been lately pointed out by the experiments of MM. Danger and Flandin.

The process is founded upon the same principle as that

* The above is an abstract from a paper in Erdmann's *Journal für Praktische Chemie*, No. 19, 1841.

adopted by M. Fuchs for the analysis of crude iron and iron ores*.

A slip of copper immersed into hydrochloric acid, sp. gr. 1·172, containing arsenic, was not acted upon, in a closed vessel, after remaining twelve hours at the ordinary temperature; if the acid was diluted with an equal quantity of water, action took place after a few hours, the arsenic being precipitated upon the copper; if the solution was exposed to the air the action occurred in a still shorter time, but if heated it took place nearly immediately, whether the acid was concentrated or diluted. The copper was covered at first with a grayish brilliant metallic coating, which upon increase of temperature, and according to the quantity of arsenic present in the solution, turned black, and finally separated in black scales.

In order to ascertain the delicacy of this reaction, a solution of one part of arsenious acid was made in 1000 parts of pure hydrochloric acid diluted with water. A portion of this solution (to which one-third of pure hydrochloric acid was added), containing 1-100,000th part of arsenic, was acted upon as soon as it was heated; at first the precipitate had the appearance of iron, but after long boiling it became black with a metallic lustre. With a solution containing 1-200,000th part, the copper was distinctly covered with arsenic after a quarter of an hour's boiling. The limit appears to be between 1-250th and 1-300,000th parts; and is therefore considerably greater than that of any other reagents, and is not likely to be mistaken for other substances.

If arsenious acid be dissolved in water, and a slip of copper immersed, no action takes place; but if a few drops of hydrochloric acid be poured upon the copper, the arsenic is immediately precipitated. A quantitive estimate of the arsenic may be made by this method, as on boiling the solution nearly the whole of the arsenic is separated from the copper, and the quantity of arsenic may be calculated from the loss of weight sustained by the copper. The arsenic obtained may either be further tested by Marsh's apparatus, or it may be heated in an open glass tube, when brilliant crystals of arsenious acid will be obtained, which may be dissolved in a weak alkaline solution and tested by the usual reagents; or the slip of copper may be heated in a glass tube with hydrogen gas, when metallic arsenic will be sublimed.

Antimony.—Copper does not precipitate this metal with the same iron-like appearance as arsenic; the precipitate has

* See Phil. Mag. for February 1841.

a less metallic lustre, and is of a decided violet colour. With a solution containing 1-200,000th part of antimony, the precipitation is so thin that the copper shines through, but has still a violet hue: upon making both precipitates they are easily discriminated by comparison. The delicacy of the reaction is equal to that of arsenic. Antimony also gives no precipitate without the presence of hydrochloric acid, but the addition of a few drops immediately occasions one.

Tin.—Copper does not occasion a precipitate in a solution of this metal containing 1-100th part to which an equal quantity of hydrochloric acid has been added, if kept from the air; on boiling the solution there are slight indications of a metallic precipitate. If exposed to the air in an open vessel, the metal is precipitated after a few days as a grayish black powder: in weaker solutions no precipitate whatever occurs.

Lead.—With a solution containing 1-500th part of lead, to which an equal quantity of hydrochloric acid was added, copper produced no precipitate, when kept free from the air; on boiling only a few spots appeared. In dilute solutions there was no action; on exposure to the air a black powder was precipitated.

Bismuth.—With a similar solution of bismuth the slip of copper was immediately covered with a grayish metallic coating, when kept free from the air; this precipitate was gradually converted into small flat crystals; on heating the solution, the copper was immediately covered with a crystalline deposit. The metal was also precipitated from very weak solutions.

Mercury.—A solution containing 1-1000th part of mercury mixed with hydrochloric acid, was immediately precipitated upon the copper as a white coating. A solution containing 1-50,000th part of corrosive sublimate without the addition of hydrochloric acid, had no action on the copper when cold; on heating it produced a golden colour; on addition of hydrochloric acid and after boiling, the copper had a gray appearance, and metallic mercury might be observed by means of a single microscope.

Silver.—A solution containing 1-1000th part of silver, to which hydrochloric acid was added, produced a white coating upon a slip of copper; after twelve hours there were small dendritical crystals formed, which were increased upon heating the solution. A solution containing 1-50,000th part of silver without hydrochloric acid, gave the copper a yellowish tinge, which disappeared upon adding hydrochloric acid, but no silver was separated with a solution containing 1-20,000th part silver; the metal was precipitated in metallic spots.

It follows from this examination:—

1st. That metallic copper is the most delicate and surest reagent for the detection of arsenic, the reaction taking place with a solution containing 1-200,000th part.

2nd. That with antimony the action is similar to arsenic, but it is not precipitated with such a metallic lustre, and differs from it by the decided violet colour produced.

3rd. That lead and tin are not precipitated either from a concentrated or dilute solution in the metallic state; this only occurs upon exposure to the air.

4th. The solution of bismuth is immediately precipitated in a crystalline state.

5th. That silver and mercury are precipitated with a silver lustre, and the reaction does not exceed a 1-20,000th part in solution.

LXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xviii. p. 241.]

June 17, 1841. — THE following papers were read, viz.—

1. "Experiments on the electric conditions of the Rocks and Metalliferous Veins (Lodes) of Longclose and Rosewall Hill Mines in Cornwall." By William Jory Henwood, Esq., F.R.S., &c., Secretary of the Royal Geological Society of Cornwall.

The experiments, of which the results are given in this paper, were undertaken with the view of determining whether it was in consequence of the imperfections of the galvanometers, or other apparatus, employed, that Mr. R. W. Fox, and other experimenters, had been unable to detect the presence of electricity in the tin veins of Cornwall. The mode of experimenting was in principle the same as that pursued by Mr. Fox, namely, that of placing plates of metal in contact with the points to be examined, carrying wires from the one to the other, and interposing a galvanometer in the circuit. The plates employed were of sheet-copper and sheet-zinc, and they were about six inches long, and three inches and a half wide. The wires were of copper, one twentieth of an inch in diameter, and the same that had been used by Mr. Fox.

The tabular results of these experiments show that both the granite and the tin vein at *Rosewall Hill* mine, and also the greenstone and the copper vein in that of Longclose, present unequivocal traces of electric currents, whether different parts of the same veins or various portions of the same rocks were examined.

It also appears, from these experiments, that the nature and positions of the small metallic plates employed materially affect, not only the intensity, but in some cases also the directions of the currents; and also that there is a considerable difference in the results when the same plates of metal are placed on different ingredients in the veins, even though these may be in immediate contact with each other.

2. "Researches in the Theory of Machines." By the Rev. H. Moseley, M.A., F.R.S., Professor of Natural Philosophy and Astronomy in King's College, London.

Of the various names, such as "useful effect," "dynamical effect," "efficiency," "work done," "labouring force," "work," which have been given to that operation of force in machinery which consists in the union of a continued pressure with a continued motion, the author gives the preference to the term *work*, as being that which conveys, under its most intelligible form, this idea of the operation of force, and as being the literal translation of the word "travail," which among French writers on mechanics has taken the place of every other.

The single unit, in terms of which this operation of force is with us measured, viz. the work of overcoming a pressure of one pound through one foot, he considers to be distinguished sufficiently, and expressed concisely enough, by the term *unit of work*, rejecting as unnecessary, and as less likely to pass into general use, the terms "dynamical unit," and "dynam," which it has been proposed to apply to it.

Having thus defined the terms *work* and *unit of work*, and paid a tribute of respect to the valuable labours of M. Poncelet in the theory of machines, and expressed admiration of the skill with which he has applied to it the well-known principle of *vis viva* under a new and more general form, the author proceeds to remark, that the interpretation which M. Poncelet has given to that function of the velocity of a moving body which is taken as the measure of its vis viva, associates with it the definitive idea of a force opposed to all change in the state of the body's rest or motion, and known as its "*vis inertiæ*," "*vis insita*," &c. The author conceives that the introduction of the definitive idea of such a force into questions of elementary and practical mechanics is liable to many and grave objections; and he proposes a new interpretation of it, viz. "that one half of this function represents the number of units of work accumulated in the moving body, and which it is capable of reproducing upon any resistance opposed to its progress." This interpretation he establishes by mechanical considerations of an elementary kind. Taking, then, this new interpretation of the function representing one half the vis viva, and dividing the parts of a machine into those which receive the operation of the moving power (the moving points) and those which apply it (the working points), he presents the principle of vis viva in its application to machines under the following form:—"The number of units of work done by the moving power upon the moving points of the machine is equal to the number yielded at the working points, *plus* the number expended upon the prejudicial resistances, *plus* the number accumulated in the various parts of the machine which are in motion." So that the whole number of units of work done by the moving power, or upon the moving points, is expended, partly in that work done at the working points, whence results immediately the useful product of the machine, and partly upon the prejudicial resistances of friction, &c. opposed to the motion of the machine in its transmission from the moving to the working points; and all the

rest is accumulated or treasured up in the moving parts of the machine, and is reproducible whenever the work of the moving power from exceeding shall fall short of that which must be expended upon the useful and the prejudicial resistances to carry on the machine.

He then proceeds to observe, that in every machine there thus exists a direct relation between these four elements,—the work done upon the moving points, that expended at the working points, that expended on the prejudicial resistances, and that accumulated in the moving elements. This relation, which is always the same for the same machine, and different for different machines, he proposes to call, in respect to each particular machine, its *modulus*; and he states the principal object of this paper (and of another which he proposes subsequently to submit to the Society) to be, first, the general determination of the modulus of a simple machine; secondly, that of a compound machine, from a knowledge of the moduli of its component elements; and, thirdly, the application of these general methods of determination to some of the principal elements of machinery, and to the machines which are in common use.

The author then states, that the velocities of the different parts, or elements of every machine are connected with one another by certain invariable relations, capable of being expressed by mathematical formulæ; so that, though these relations are different for different machines, they are the same for the same machine. Thus it becomes possible to express the velocity of any element of a machine, at any period of its motion, in terms of the corresponding velocity of any other element. Whence it results that the whole vis viva of the machine may at any time be expressed in terms of the corresponding velocity of its moving point (that is, the point where the moving power is applied to it), and made to present itself under the form $V^2 \Sigma \omega \lambda^2$, where V represents the velocity of the moving point of the machine, ω the weight of any element, and λ a factor determining the velocity of that element in terms of the velocity V of the moving point. Substituting this expression for the vis viva or accumulated work in the modulus and solving in respect to V, an expression is obtained, whence it becomes apparent that the variation of the velocity V of the moving point, produced by any given irregularity in the work done upon the moving or working points, will be less, as the factor $\Sigma \omega \lambda^2$ is greater. This factor, determinable in every machine, and upon which the uniformity of its action under given variations of the power which impels it depends, he proposes to introduce into the general discussion of the theory of machines as the *coefficient of equable motion.*

He then proceeds to investigate general methods for the determination of the modulus of a machine, deducing them from those general relations which are established by the principles of statics, between the pressures applied to the machine, in its state bordering upon motion.

That he may escape that complication of formulæ which results from the introduction of friction, by the ordinary methods, into the consideration of questions of equilibrium, the author calls to his aid

a principle, first published by himself in a paper on the 'Theory of the Equilibrium of Bodies in contact,' printed in the fifth volume of the Cambridge Philosophical Transactions, viz. "that when the surfaces of two bodies are in contact under any given pressures, and are in the state bordering upon motion, on those surfaces, then the common direction of the mutual resistances of the surfaces is inclined to their normal at the point of contact at a certain angle, given in terms of the friction of the surfaces by the condition that its tangent is equal to the coefficient of friction." This angle the author has called "*the limiting angle of resistance*:" it has since been used by other writers under the designation of the "*slipping angle*."

He next proceeds to determine the modulus of a simple machine, moveable about a cylindrical axis of given dimensions, and acted upon by any number of pressures in the same plane. He applies the principle last stated to determine the general conditions of the equilibrium of these pressures, in the state bordering upon motion by the preponderance of one of them; and, solving the resulting equation in respect to that one pressure by the aid of Lagrange's theorem, he deduces immediately the modulus from this solution by principles before laid down. The modulus, thus determined, he then verifies by an independent discussion of that particular case in which three pressures only are applied to the machine, one of which has its direction through the centre of the axis.

This solution he next considers more particularly with reference to a machine moveable about a fixed axis under one moving and one working pressure (their directions being any whatever) and its own weight; which last is supposed to act through the centre of the axis. He shows that it is a general condition of the greatest economy in the working of such a machine, that the moving and working pressures should have their directions, one of them upwards, and the other downwards, and that both should therefore be applied on the same side of the axis of the machine. He moreover shows that if the direction of one of these pressures be given, there is then a certain perpendicular distance of the other from the centre of the axis, and a certain inclination of its direction to the vertical, at which perpendicular distance, and which inclination, this pressure being applied, the machine will yield a greater amount of work, by the expenditure of a given amount of power, than it will yield under any other circumstances of its application: so that this particular distance and inclination are those whence results the most economical working of the machine.

Professor Moseley then commences his application of these general principles to elementary machines with the pulley. He establishes the modulus of the pulley under any given inclination of the parts of the cord passing over it, taking into account the friction of the axis, the weight of the pulley and the rigidity of the cord, and adopting, with respect to the last element, the experiments of Coulomb. This general form of the modulus of the pulley he applies, first, to the case in which both strings are parallel, and inclined to the vertical at any angle; secondly, to the case in which they are equally inclined

on either side of the vertical; thirdly, to the case in which one is horizontal and the other vertical; and, fourthly, to that in which both are horizontal. He concludes his paper by a deduction from this last case of the modulus of a system of any number of pulleys or sheaves, sustaining among them the weight of any given length of rope horizontally.

3. "On the Nervous Ganglia of the Uterus." By Robert Lee, M.D., F.R.S.

The author, in a paper which was read to the Royal Society on the 12th of December, 1839*, had described four great plexuses under the peritoneum of the gravid uterus, having an extensive connexion with the hypogastric and spermatic nerves. From their form, colour, general distribution, and resemblance to ganglionic plexuses of nerves, and from their branches actually coalescing with those of the hypogastric and spermatic nerves, he was induced to believe, on first discovering them, that they were ganglionic nervous plexuses, and that they constituted the special nervous system of the uterus. He states in the present paper, that subsequent dissections of the unimpregnated uterus, and of the gravid uterus in the third, fourth, sixth, seventh, and ninth months of pregnancy, have enabled him not only to confirm the accuracy of his former observations, but also to discover the important fact, that there are many large ganglia on the uterine nerves, and on those of the vagina and bladder, which enlarge with the coats, blood-vessels, nerves, and absorbents of the uterus during pregnancy, and which return, after parturition, to their original condition before conception took place. The author next proceeds to describe the two great ganglia situated on the sides of the neck of the uterus, in which the hypogastric and several of the sacral nerves terminate, and which he calls the *hypogastric*, or *utero-cervical ganglia*. In the unimpregnated state, they are of an irregular, triangular, or oblong shape, about half an inch in the long diameter, and always consist of grey and white matter, like other ganglia. They are covered by the trunks of the vaginal and vesical arteries and veins; and each ganglion has an artery of considerable size, which enters it near the centre and divides into branches, accompanying the nerves given off from its anterior and inferior borders. From the inner and posterior surface of each of these ganglia, nerves pass off, which anastomose with the hæmorrhoidal nerves, and ramify on the sides of the vagina, and between the vagina and rectum. From the inferior border of each hypogastric ganglion several fasciculi of nerves are given off, which pass down on the sides of the vagina, and enter some large flat ganglia, midway between the os uteri and ostium vaginæ. From these vaginal ganglia innumerable filaments of nerves, on which small flat ganglia are formed, extend to the sphincter, where they are lost in a white dense membranous expansion. From this great web of ganglia and nerves numerous branches are sent to the sides of the bladder, and enter it around the ureter. All these nerves of the vagina are accompanied with arte-

[* An abstract of this paper appeared in Phil. Mag. S. 3. vol. xvi. p. 590. —EDIT.]

ries; and they often form complete rings of nerve around the trunks of the great veins.

The author then describes the nerves which are given off from the anterior margin of each hypogastric ganglion, some of which pass on the outside of the ureter and others on the inside, and meet in front of the ureter in a ganglion, which he calls the *middle vesical ganglion.* There are other two ganglia, he states, formed on these nerves; one between the uterus and ureter, and the other between the ureter and vagina. These he calls *the internal and external vesical ganglia.* Not only is the ureter inclosed within a great ring of nervous matter, which, he says, resembles the œsophageal ganglia in some of the Invertebrata; but the trunks of the uterine artery and vein are likewise encircled by a great collar of nervous matter, between which and the hypogastric ganglion several large and some small branches pass.

The author gives the following description of the vesical ganglia. The internal vesical ganglion, which usually has a flattened or long bulbous shape, is formed entirely upon the nerves which pass from the hypogastric ganglion, and run between the uterus and the ureter. It has an artery which passes through its centre. It first gives off a large branch to the ring of nerve or ganglion which surrounds the uterine blood-vessels; it then sends branches to the anterior part of the cervix uteri, and afterwards a great number of small filaments to the muscular coat of the bladder behind, where it is in contact with the uterus; and it then sends forwards a large branch, which terminates in the middle vesical ganglion. This ganglion sends off a great number of large nerves to the bladder. Some of these accompany the arteries, and can be seen ramifying with them upon the whole of the superior part of the organ, even to the fundus. Filaments of these nerves, scarcely visible to the naked eye, are seen in one of the preparations ramifying upon the bundles of muscular fibres, occasionally forming loops and inclosing them, or passing down between them to the strata of fibres below. Some of the smaller branches of the middle vesical ganglion do not accompany the arteries, but are distributed at once to the parts of the bladder around the ureter.

The external vesical ganglion is formed entirely upon the nerves which proceed from the hypogastric ganglion, and pass on the outside of the ureter. This is a small thin ganglion, the branches of which are sent immediately into the muscular coat of the bladder. It usually sends down a long branch to anastomose with the nerves issuing from one of the vaginal ganglia.

From the inner surface of each hypogastric ganglion numerous small white, soft, nerves pass to the uterus, some of which ramify upon the muscular coat about the cervix, and others spread out under the peritoneum to coalesce with the great ganglia and plexuses situated on the posterior and anterior surfaces of that organ. Large branches also go off from the inner surface of the ganglion to the nerves surrounding the blood-vessels of the uterus, which they accompany in all their ramifications throughout its muscular coat.

This paper is illustrated by two drawings, in which the hypogas-

tric, vaginal, vesical and uterine ganglia are delineated in the fourth month of pregnancy, and also the plexuses of nerves on the anterior surface of the uterus.

From an examination with the microscope of portions of the plexuses under the peritoneum of a gravid uterus in the ninth month, which had long been immersed in rectified spirit, Professor Owen and Mr. Kiernan inferred that they were not nervous plexuses, but bands of elastic tissue, gelatinous tissue, or cellular membrane.

The author concludes his paper with a letter from John Dalrymple, Esq., containing the results of the observations he had made with the microscope on the uterine nerves in the recent state. Filaments of the nerves which surrounded the ureter, and which were situated upon the body of the uterus, were submitted to the microscope. The instrument employed was a very powerful object-glass, whose focus was the eighth of an inch, made by Ross. Mr. Dalrymple found that it was impossible, even with the most careful dissection, to detach any filament of nerve without including a quantity of cellular and elastic tissue; so that although the tubular portion indicating the nerve was distinct, yet it was surrounded by innumerable extremely minute threads coiled and contorted, such as those which constitute the component of elastic tissue, and the ultimate element of cellular membrane. Under slight pressure, however, the tube was plainly discernible, and was found to contain granular matter, not uniformly distributed, but collected in minute masses at intervals. Small blood-vessels were also here and there seen, with blood-discs within them, which served to indicate the difference between the nervous and vascular tubes, and thus to avoid the possibility of error. Being, however, aware that some of the most distinguished foreign microscopical anatomists had differed as to what was the real characteristic of the nerves of the sympathetic system, and feeling, from this discordance of opinion, that there was no absolute test, or at least none which was not open to cavil, Mr. Dalrymple thought of making a comparison of the uterine nerves with those that undeniably belonged to the ganglionic system. He therefore traced some nerves on the surface of the stomach up to the great ganglion that gave them origin; and he selected some also from the small intestine. These he submitted to the same microscopical power, and under the same circumstances of light, and pressure, and medium. In all of these he observed the tubular part filled with granular matter, and similarly collected in minute masses. He also observed that each tube was surrounded by the minute serpentine threads before described. In fact, so closely did they agree in every particular with the appearances presented by the uterine nerves, that it would have been impossible to distinguish the one from the other.

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[Continued from p. 402.]

March 24, 1841.—The reading of the paper on Russia, by Mr. Murchison and M. E. de Verneuil, was resumed and concluded.

The Memoir, of which the following is an abstract, is the result of a journey through the Northern and Central Governments of Russia in Europe, made during the summer of 1840, a verbal account of some of the chief points of which, accompanied by a new geological map of those regions, was offered to the public at the meeting of the British Association for the Advancement of Science, in September 1840.

Introduction.—The authors preface their memoir with a sketch of the condition of geological knowledge concerning the flat and central countries of Russia in Europe anterior to their visit, and show that the early efforts of Strangways* had not been followed up by any connected attempt to establish the classification and succession of the older sedimentary deposits on the true principles of the order of their superposition, and their distinctions by organic remains. They point out, however, that certain elements of the subject had been prepared; first, by the map and descriptions of Strangways; secondly, by the publication of the palæontological works of Fischer de Waldheim, Pander, and Eichwald; thirdly, by the recent researches of Colonel Helmersen in the Waldai Hills; and fourthly, by the important zoological distinctions indicated by M. Leopold de Buch, who, on hearing of the plan of the voyage of the authors, expressed his belief (from the examination of certain fossils alone) that the triple subdivision of the *palæozoic* rocks into the Carboniferous, Old Red, and Silurian systems, as indicated by Mr. Murchison†, would be found to prevail in Esthonia, Livonia and Courland.

After alluding to the vast importance to the Russian empire of a correct knowledge of the subsoil of these flat regions, the authors explained the scheme which they had devised, before they left their own countries, for ascertaining the data required. Aware of the two great difficulties which are opposed to the examination of this region,—the slight altitude of the masses above the sea, and the vast quantity of drift or the slight superficial detritus which obscures the fundamental rocks,—they overcame these obstacles by examining, in succession, the banks of the rivers between the longitude of St. Petersburgh and of Archangel, which, flowing from N.N.W. to S.S.E., or transverse to the only apparent lines of elevation, might be expected to offer the evidences required. They also ascended the great Dwina, from the White Sea to Oustiug Veliki; and afterwards extended their researches to the south of Nijnii Novogorod, in order to determine the relations of the secondary rocks to those older deposits with which they had become familiar.

In terminating these introductory explanations, the authors dwelt with pleasure on the valuable assistance they had received, particularly in the early part of their tour, from the Baron A. de Meyendorf‡, now executing, by order of his Imperial Majesty, a statistical survey of Russia, who endeavoured to combine geology and natural history with the chief object of his expedition by attaching to it two excellent naturalists, Count Keyserling and Professor Blasius. They

* Geol. Trans., First Series, vol. v.; Second Series, vol. i.

† Silurian System and map.

‡ Assisted by M. Zenofief.

further testified their warm thanks to the Russian minister, the Count de Cancrine, who specially aided this geological inquiry, and they also acknowledged their obligations to Count Nesselrode, Count Alexander Strogonoff, Baron Humboldt, Baron Brunnow, and General Tcheffkine. They further expressed their sense of the value of the services of a zealous young geologist, Lieutenant Koksherof, without whose aid the authors could not have accomplished their task. A geological map and sections illustrated the description, and the characteristic fossils of each group were laid upon the table.

Crystalline Rocks, Metamorphic Rocks, Trap Rocks, Physical Geography, &c.—Before they proceed to describe the sedimentary deposits in their order from S. to N., or from the older to the younger strata, the authors mention some peculiar varieties of gneiss which occupy the little islands of the White Sea near Onega, one of which is charged with garnets. They then give a brief sketch of the altered condition of the sedimentary strata on the western shore of the lake Onega, where they are pierced by masses of greenstone and trappean conglomerate.

A few words explain how the Waldai Hills, the great watershed of Central Russia, afford the best means of reading off the succession of the older strata. The rivers Msta, Wolkoff, Siass, &c., which flow from the south to the north, having short courses, necessarily occupy deeper rents, and therefore expose on their banks better sections than those streams, which, descending on the other side of the crest, glide along on a very slightly inclined plane to the south. By examining the banks of the north-flowing rivers, the older formations were found to succeed each other in the following *ascending order* :—

1. *Silurian Rocks.*—The oldest sedimentary deposits of Russia (those on which St. Petersburgh is situated) are clays, sandstone, limestone and flagstone, which from their position and organic remains are considered the equivalents of the Silurian system of the British Isles. The detailed order of these beds was long ago given by Strangways; but at the early day when he wrote, the study of organic remains was not sufficiently advanced to enable him to determine the exact place of these beds in the geological series, nor to point out their true relations to the adjacent masses. Many of the fossils have since been described by the native authors, Pander and Eichwald, and recently some very characteristic forms by M. de Buch.

The Silurian deposits consist in ascending order of *blue clay, intermediate grit,* and *overlying limestone*, &c. In the first of these no organic remains have yet been found; and the intermediate sandstone or grit is alone distinguished by a remarkable form unknown in western Europe (*the Ungulite*), which the authors consider to be nearly allied to *Orthis*. They likewise discovered in this grit one small shell resembling a *Pecten*. In the limestones, and certain overlying flagstones first described on this occasion, organic remains abound; and they agree well in the leading characters on which the Silurian system was established, viz. that the forms of *Trilobite,*

Orthoceratite, and *Orthis* are distinct from the types of the overlying members of the palæozoic series.

The most prevalent fossils are the *Orthoceratites vaginatus*, *Asaphus expansus*, *Illænus crassicauda*, the peculiar Crinoidean *Spheronites* (allied to the *Ischadites* of the Upper Silurian rocks), and a vast profusion of many species of *Orthis*. Although, upon the whole, the Silurian fossils of Russia differ more than those of Sweden from British species of the same age (as might indeed be expected from their more remote distance), certain shells are identical with those published from England; among which are enumerated, *Leptæna depressa* (*L. rugosa*, Dalm.), *Leptæna sericea*, *Lingula Lewisii*, *Orthis canalis* (*O. elegantula*, Dalm.), &c.; and according to M. Eichwald, two or three species of Trilobites*.

With the exception of some very trivial dislocations in the low hills south of St. Petersburgh, the Silurian rocks are so uniformly horizontal, that in the fine quarries on the banks of the Wolkoff, the authors were able to prove a difference of 2° or 3° to the S.S.E. only by pouring water on the surface of the rocks.

These Silurian deposits occupy the islands of Öland, Gothland, &c. in the Baltic, and trend along the shores of Esthland in a broad band from W.S.W. to E.N.E., till they are lost under vast heaps of granitic detritus between the lakes Ladoga and Onega. Near the latter, these deposits are deflected to the north, and there meet with great ridges of trappean rocks, which run from N.N.W. to S.S.E. In that region all the deposits are in a metamorphic condition; the limestones present no distinct traces of fossils; and the authors having satisfied themselves that there was no chance of observing any further evidence of a descending order between such rocks and the great primarized granitic chain of Scandinavia and Russian Lapland, the boundary of which they coasted, confined their attention to the *ascending* order of the strata, which is clearly exhibited on the banks of the Wolkoff and at other places.

2. *Old Red, or Devonian System.*—That the inferior strata are the true equivalents of the Silurian system, was determined not only by their aspect and fossil contents, but by their being overlaid by other rocks which are completely identical with the "Old Red System" of the British Isles, as defined by Mr. Murchison†. This system is of great extent in Russia. It passes from Livonia by the lakes of Ilmen and the Waldai Hills, and is extended over a vast region to the N.E., where it constitutes a large portion of the shores of the White Sea. This system consists of flagstone, clays, marls, cornstones and sandstones, the whole bearing a considerable resemblance to some red deposits of the same age in our isles, but differing by containing copious *salt springs* and much *gypsum*. It was the occurrence of so much salt and gypsum that led previous writers to consider these deposits an equivalent of our new red system, which,

* See Professor Eichwald's work, published since the authors' visit to Russia, entitled 'Silurische-Schichten-system in Esthland.'

† See Silurian Researches, p. 165, and Table with the Map.

being found to contain the same minerals in the western parts of Europe, had been even termed by some, the saliferous system. That the red deposits (red and green) are, however, the true equivalents of our old red sandstone, is demonstrated, not only by order of superposition, but also by the many organic remains which they offer. Fishes are the most distinguishing fossils of this great Russian system, and among these are species (notably the *Holoptychius Nobilissimus*, Murchison, with the *Coccosteus, Diplopterus* and *Ctenoptychius* of Agassiz), forms which occur in deposits of the same age in Scotland. The fishes are in abundance, and a work, illustrative of them, is now preparing by Professor Asmus, of Dörpat, near which University they abound. The authors have traced these fish-beds for a great distance, occupying several stages in the system, and each stage characterized by peculiar species of ichthyolites.

The zoological contents of this system are also of great value in illustrating and confirming the palæozoic classification proposed by Messrs. Sedgwick, Murchison and Lonsdale; or in other words, the evidences found in Russia leave no doubt that the old red and Devonian systems of rocks are identical. The *Orthis subfusiformis, O. striata, Spirifer calcarata, S. trapezoidalis, Productus caperatus, Terebratula prisca* (large var.), and *Serpula omphaloides*, shells distinct from those of the carboniferous system, but similar to those which occur in Devonshire, Westphalia, Belgium, and other places (in deposits which have been shown by these authors to be of the age of the old red sandstone), are found in Russia *in the same beds with the fossil fishes of the old red sandstone* of the British Isles.

Still more striking, observe the authors, are these cumulative proofs, when it is stated, that although in France and Germany there are scarcely any lithological equivalents for the British old red system, yet, that in extending researches far to the east, this member of the series is found to resume very many of the same mineral characters which distinguish it in the central and northern parts of the British Isles; and then under *similar conditions* it contains the ichthyolites of the British deposits.

3. *Carboniferous System.*—In the northern regions of Russia, the lower or calcareous part only of the carboniferous system exists, which in the Waldai Hills, near Wytegra, on the Onega, and in many other places, is seen to overlie the old red sandstone. The inferior beds consist of incoherent sandstones and bituminous shale, which sometimes contain thin beds of *impure pyritous coal*, and impressions of several plants well known in the carboniferous system of our own islands. These are surmounted by various bands of limestone, the lowest of which only have occasionally some mineralogical resemblance to the mountain limestone of Western Europe; other beds being lithologically undistinguishable from the magnesian limestone of England; some from a pisolite; a third and very prevalent band of considerable thickness is milk-white, and not more compact than the calcaire grossier of Paris. This white Productus limestone was traced by the authors from the neighbourhood of Moscow to beyond Archangel (and they ascertained that it ranged

far into the country of the Samoiedes), a distance of not less than 1000 miles. This formation has also a mineral resemblance to chalk, in being loaded with thin bands of flints, sometimes concretionary, in which shells and corals occur. Associated with this formation, on the banks of the Dwina, about 200 wersts above Archangel, and south of Süsskaia, are splendid bedded masses of white gypsum, which, for many miles, present at a little distance all the appearance of white limestone*. With these grand gypseous deposits, in which are occasionally large concretions, two or three thin bands of limestone alternate, in one of which the authors detected fossil shells (*Avicula*) which are new to them. Other peculiar bands near Ust-Vaga, which are rather higher in the series, contain a *Productus* approaching to *P. scabriculus*, with *Pectens* and *Corals*.

The carboniferous limestone of Russia is highly fossiliferous, and from the normal and unaltered condition of most of the beds, the fossils are generally in an excellent state of preservation. Among them are many well-known British species, the lower beds being distinguished by the large *Productus hemisphericus* so well known in the same lower beds of England and Scotland; and the white beds being loaded with many of the species published by Fischer, Phillips and Sowerby, such as *Productus Martini*, *P. punctatus*, *Sanguinolaria sulcata*, *Spirifer Mosquensis*, *Cardium alæforme*, *Cidaris vetustus*, together with the abundant and characteristic Russian coral, *Chætites radians* (found, according to Mr. Lonsdale, in the carboniferous limestone of Bristol, &c.), and the *Lithostrition floriformis*, one of the most characteristic fossils of the English carboniferous limestone, &c.

Owing to its mineral aspect, the age of this rock had, till within the last year, been misunderstood; but Colonel Helmersen having observed its position in the Waldai Hills and its association with certain beds of coal, and having ascertained the nature of the fossils through the examination of M. von Buch, he first gave out in Russia, that it must be considered the true mountain limestone. The authors have completely confirmed this view, by ascending and descending sections, and have largely extended it.

Newer Red Formations.—The manner by which the authors were led to believe in the existence of newer red deposits, forming a vast basin in the governments of Vologda, Nijnii, Kostroma, is explained at some length, by describing the ascending section of the Dwina, and by details relating to the structure of the banks of the rivers Volga, Okka, &c. They show that, although this great red series of the central government agrees with that of the north, in containing salt and gypsum, yet that it differs from the "old red" group in the lithological and zoological character of its marls, limestones, and fine conglomerates, none of the fishes or organic remains before alluded to being anywhere discoverable. In expressing their suspicion that this newer red system may be found eventually to contain the *equivalents* of the upper coal measures, lower new red sandstone

* See M. Roberts's account of these white cliffs, which he supposed to be limestone.—*Bulletin de la Soc. Géol. de France*, 1840.

(*rohte-todte liegende*), magnesian conglomerate, zechstein, and the Trias of German geologists, the authors reserve their opinions on such details until they have accomplished a tour to the Ural Mountains, on the western flanks of which they hope to detect the evidences required; it being very difficult to trace the exact sequence in the flat and obscure regions over which they followed these deposits to so wide an extent.

Oolitic or Jurassic Series.—Certain rocks of the oolitic series have been long known to exist in the centre of Russia, and some of the fossils of this series were sent to England by Mr. Strangways.

The beds of black shale which rest at once on the great red formation along the banks of the Volga, between Kostroma and Nijnii Novogorod, belong unquestionably to the middle oolite, as they contain *Ammonites* and *Belemnites*, closely approaching, if not identical in species with those of the Oxford clay and "Kelloway Rock" of Smith. Other fossils found near Jelatma, Kacimof and Moscow exhibit close relations to the fauna of the Lias as well as to that of the middle and lower oolite. Having examined a suite of specimens from Moscow, Professor Phillips confirms the views of the authors, who are disposed to think that the middle and lower oolite, as well as the Lias, are all represented in Central Russia simply by beds of black shale with subsidiary courses of oolitic marlstone, concretions, &c. Near Moscow these shales repose directly and conformably upon the carboniferous limestone. Among the fossils of the group on the Volga and the Okka are *Ammonites flexistria*, *A. Gulielmi*, *A. Königii*, *A. sublævis*, with *Gryphæa Maccullochii*?, &c. Among the fossils from Moscow are *Ammonites* of many species, some of which are figured by Fischer, others are described by Professor Phillips, for this memoir. *Belemnites absolutus* (*B. sulcatus*, Miller); *Serpula tetragona*, Sow.; *Amphidesma? donaciforme*, Phill.; *Lima proboscidea?*, Sow.; *Pecten Fisherii, N.S.*, *Inoceramus dubius*, Sow.; (*P. rugosus*, Fischer) *Terebratula serrata*, Sow.; *T. acuta*, Phill. These forms characterize the lower oolite and lias of the British Isles.

Ferruginous Sand.—The shales of the oolitic series are covered by ferruginous sands, occasionally green, which contain large flattened concretions of grit (the Moscow millstones); but never having observed fossils in this rock, the authors are unwilling as yet to hazard an opinion regarding its age. With the exception of certain very recent deposits, these grits are the youngest solid strata in the northern half of Russia in Europe.

Chalk.—The cretaceous system is largely developed in the south, near Simbirsk, and in the Crimea; but on this occasion the authors did not extend their tour to the chalk districts.

Tertiary Deposits.—The white shelly limestone of Crimea, and its relations to the underlying chalk, have already been described by one of the authors*. Such deposits have not yet been discovered in any of the northern or central regions of Russia.

Post Pleiocene (*Pleistocene*).—It was formerly the general belief,

* M. E. de Verneuil.

that the great masses of superficial detritus, whether clays, sands or blocks, which cover so very large an area of the northern region, were all referable to one epoch (diluvian) in which the bones of great extinct quadrupeds were also imbedded. The duration of their journey was not sufficient to enable the authors to make many distinctions of age between these different masses; but they have commenced this division by the discovery of beds of clay and sand on the banks of the Dwina and Vaga, upwards of 200 miles south of the White Sea, which contain *twenty-two species* of shells, many of which still preserve their colours, and which, having been referred to Dr. Beck, of Copenhagen, have been pronounced by him to be all of modern *northern* species. Mr. Lyell states that they are identical with the Uddevalla group described by him in Sweden. Mr. Smith adds, that these shells are nearly all the same as those which he has found in various ancient elevated sea bottoms around the coasts of Scotland. In referring twenty of these to modern arctic species, Mr. G. Sowerby doubts if a certain *Mya* has ever been found recent, and states that a *Cardium*, approaching to *C. ciliatum*, is different from any northern form he is acquainted with, and near to certain Australian types. This discovery, in which they were assisted by Count Keyserling, who accompanied the authors in their tour to Archangel, is conceived to be of high geological interest, as it demonstrates that, during the *quasi* modern period, the whole of the vast flat country of north-eastern Russia was beneath the sea for a considerable time, the eastern boundary of that sea being probably the slopes of the Ural Mountains.

Drift and Erratic Blocks.—Overspreading all the formations, and greatly obscuring them, is a vast mass of detritus, the large granitic and other crystalline blocks of which have excited much attention, from the days of Pallas to the present time. This detritus, the blocks of which have all been derived from the north, is shown to have been deposited *under the sea*, or in other words, upon a sea bottom, since it covers the above-mentioned shells.

Notwithstanding the obscuration occasioned by this wide-spreading drift, it is stated that the nature of the subsoil, or fundamental deposits, can often be surmised from the colour of the superficial clay and sand, and the materials of small detritus, the surface of the Silurian zone being grey, that of the old red, red; whilst the cover of the carboniferous limestone is often charged with many broken flints derived from the underlying beds of that formation, some of the siliceous fragments of which have been transported further southwards, and spread over the regions occupied by the newer red and oolitic deposits. Thus, as all the larger and harder blocks can be shown to have been carried from the mountains on the N.N.W., so in passing to the S.S.E. the finer ingredients, or matrix of the detritus, is found to change by the successive additions of materials derived from the denudation of the different members of the palæozoic series. There is no instance of any substance having been transported from S. to N., except by the modern action of streams, and by local causes dependent on the present configuration of the

land. Near Nijnii Novogorod large blocks of a very peculiar trappean conglomerate were detected, which had been derived from a rock *in situ* N. of Petrazowodsk, a distance of nearly 600 miles. In endeavouring to account for the immense distances to which these blocks had been transported, the authors expressed their belief that they had been floated in former icebergs, which breaking loose from *ancient glaciers, which they suppose may have existed in Lapland and the adjacent tracts*, were dislodged upon an elevation of the northern chain, and impelled southwards *into the sea* of that period, in which the post pleiocene shells, to which allusion has been made, were accumulated. In the relation of the blocks to the sea shells, they conceive that Central Russia presents an exact parallel (though on a much grander scale) to the phænomena described by one of the authors in the central counties of England, where a similar collocation was accounted for, by supposing that the northern blocks were borne thither in vessels of ice, which in melting dropped them upon what was then a *sea bottom**.

Glacial Action.—After alluding to the works of Sefström† and Bötlingk upon the supposed "diluvial" currents of Scandinavia and Lapland, as evidenced by the parallel striæ and polishing of the surface of the hard rocks of these regions, the authors describe the most southerly of the scratches, which came under their notice near Petrazowodsk, on the lake Onega, no such markings having anywhere been observed in Central Russia. They then examine the applicability of the glacial theory, as proposed by M. Agassiz, to the tracts of Russia under review. Starting from what they conceive to be an axiom, that the advance of every modern glacier depends upon the superior altitude of the ground behind it, they show, that if certain parallel striæ, observed by M. Bötlingk, and others noted by themselves, are to be taken as proofs of the *overland* march of glaciers, such bodies must often have been propelled *from lower to higher* levels. For the proofs of this they refer to the eastern sides of the Bothnian Gulf, where M. Bötlingk found the striæ ("*diluvial schrammen*") directed in common with the boulders from N.W. to S.E.; and yet any glaciers which bore these blocks must have advanced from Scandinavia, across the Baltic Sea, and then have ascended the rocky tract in question. Again, near Petrazowodsk, in the isles of the lake Onega, the authors observed such striæ exactly parallel to the major axis of the lake, N.N.W. and S.S.E., even from a good many feet under the clear fresh water, and thence rising to the height of twenty feet above the summer level of the lake on the sloping surfaces of the rock. They then argue, that in this tract there are no hills of sufficient altitude on the N.N.W. to account for the determined forward direction to the S.S.E.; and as a still further reason for rejecting the application of the "Alpine glacial theory" to this country, they add, that as the striæ in one region have all a given and parallel direction, so must the supposed glacier not only

* Silurian System, p. 535 *et seq.*

[† A translation of M. Sefström's paper on this subject will be found in Part ix. of Taylor's Scientific Memoirs.—Edit.]

havc moved on as it wcre without a cause, but also have maintained an ineredibly enormous advaneing front of many hundred miles in length!

Without pretending to offer a eomplete solution of so diffieult a problem, and after stating that many additional and even experimental researehes are required in relation to the power of water, drift, and ice, they cannot avoid suggesting as a probable explanation of the ehief phænomena in the North of Russia, that eurrents strongly determined in given directions by the elevation of the northern eontinental masses, might dislodge and set in movement icefloes and detritus, whieh, *grating upon the bottom of a sea, may have produced the parallel striæ.* They are the more confirmed in this hypothesis, by the faet, that the longer axes of the lakes and stony ridges of Northern Russia have generally the *same direction*; so that thc supposed ieebergs and *land detritus* would neeessarily be borne in that direetion. By adopting this view, the existenee of the post pleioeene shells of the Vaga and the Dwina, and their relations to the overlying drift from the North, are in harmony; and whilst admitting so mueh of the glaeial theory as to allow, that in former days glaciers probably advaneed further to the South and oeeupied many insulated tracts, and to a much greater extent than at the present day, the geologist, thcy coneeive, is alone called upon to define and limit the area of *land* in Scandinavia and Lapland, once covered with solid iee, in doing which he must of eourse exelude from such ageney the vast countries now covered by erratie bloeks, which he ean demonstrate were deposited upon the *bottom of the sea.*

Angular block-ridges on lake and river Banks.—On the western shore of the great lake of Onega, the attention of the authors was directed, by Colonel Armstrong*, to three parallel ridges of large angular blocks of hard grit (old red sandstone?), whieh oecur at heights, varying from 20 or 30 to 150 feet or more above the level of the water. As these bloeks were identieal in eomposition with the solid subjaeent roek, and also quite angular, it was at onee evident that they had not been drifted, but simply rent from the solid roek whieh forms that side of the lake. On a first inspection, the authors were disposed to think that these appearanees might have been caused by upheaving or vertical shoeks of earthquakes, which they presumed might be among the last signs of the great igneous aetion whieh had onee been so dominant in these northern traets; and they were unable to account for them satisfaetorily, until they detected the results of modern aetion of river iee, whieh completely explained the lacustrine ease.

About 80 milcs above Arehangel they met with a ridge of large angular blocks of white limestone piled up between the road on whieh they travelled and the river edge, and about 20 or 30 feet above the stream. Having ascertained that this great river was periodieally subjeet to oeeasional extraordinary rises in the spring, and that on those oeeasions it bursts and throws up upon its banks blocks

* Director of the Imperial Iron Foundries of Petrazowodsk.

of ice to heights of 20 or 30 feet above its ordinary level, they had at once a solution of the phænomenon; for the blocks of white limestone had evidently formed parts of the subjacent strata, which, projecting into the mud and water on the edge of the Dwina, had been first entangled in ice, and rent off at their natural joints upon the expansion of the ice by which they were upheaved into their present position, taking their present irregular talus shape when the ice melted away from them. Believing, therefore, that the angular ledges on the lake of Onega were similarly formed, the authors see in them the proofs of the lakes of Northern Russia having formerly stood at much higher levels, from which the waters, they suppose, have been let off by successive elevations of the land; and they further think, that the diminution of shallow lakes, and the conversion of marshes into land within the historic period in Northern Russia, strongly corroborate the rise of this portion of the earth.

Conclusion.—In recapitulating the chief point of the first and practical part of their Memoir, wherein they establish, they trust, on a sound basis, the general classification of the Palæozoic Rocks of Russia in Europe, the authors remark, that the fact of some of the deposits of such high antiquity being found to stretch in horizontal and almost unbroken sheets over spaces of a thousand miles in length, in a very slightly solidified or lapidified state, is the more interesting when coupled with the absence, throughout the same regions, of all plutonic or igneous rocks. This phænomenon must, it is conceived, exercise considerable influence upon geological theory, it being now apparent, that the lithological nature of the most ancient subsoil of Russia in Europe is such as to compel geologists to reject the conclusion, that in proportion to their antiquity the strata have been hardened or crystallized by any general radiation of central heat; for in these wide tracts such crystalline and hardened state is clearly seen to be purely metamorphic, and dependent exclusively on the vicinity of rocks of igneous protrusion, in receding from which to the South all the strata described are at once found in their normal soft condition.

In taking leave of the Society, the authors explained some of the chief objects of their journey to the Ural Mountains, Orenburg, &c., on which they were about to proceed.

Note.—After these sheets were sent to press, Mr. Murchison received letters from his friends and fellow-travellers, the Baron A. de Meyendorf and Count A. Keyserling, in which the researches of these gentlemen in the South of Russia are explained. These letters communicate important additions to the results already offered to the Geological Society, particularly in regard to the extension and development of the carboniferous system. The geological map which has been prepared by their labours, and from those of other Russian authorities, agrees with that of Mr. Murchison and M. de Verneuil, exhibited to the Society, in the fundamental classification of the rocks which occupy the northern and central governments of Russia, and in the lines of demarcation between the *Silurian*, *Devo-*

nian or *Old Red, Carboniferous, Newer Red,* and *Oolitic Systems*; but it is copiously enlarged, by showing the extension of the carboniferous system over a very wide area, ranging from near Witepsk, by the south of Tula and Kaluga, to the S.E. of Cazan. A vast spread of chalk and tertiary deposits directly overlies these carboniferous limestones, which rise again from beneath these younger formations in the great carbonaceous tract of the Donetz, the southern edge of which consists of the granitic steppe. A section made by Count Keyserling and Professor Blasius to the south of Kaluga, indicates a succession from what these naturalists believe to be the lower beds of the carboniferous limestone, containing *Spirifer Mosquensis,* into superior strata of sand and shale with coal, subordinate to bands of limestone containing the *Productus hemisphericus,* the coal being associated with much red earth, and overlaid by the upper carboniferous limestone. They also express their belief that the millstone grits which have been alluded to near Moscow must be considered of tertiary age, as similar beds overlie true chalk.

Mr. Murchison takes this opportunity, in the name of his friend M. de Verneuil and himself, of recording his sense of the value of the additional data which are due to the labours of Baron de Meyendorf and his associates, and trusts that after an exploration of the flanks of the Ural, and other tracts near Orenburg and in the South, all the chief facts will have been obtained for the construction of a general geological map of Russia in Europe.

Count Keyserling, who has traced the shales with *Ammonites* near Ust-Sisolsk (N. Lat. 61°, E. Long. 51°*), has indeed contributed most powerfully to these results, both by his patient observation, sound knowledge of natural history, and by his barometrical admeasurement of heights,—a point of great geological importance in those central parts of the country where the strata are not deranged. By one of his observations, it appears, that the younger pleiocene deposits on the Dwina, which he detected in company with M. de Verneuil and Mr. Murchison, are about 150 feet above the White Sea. Count Keyserling, now at St. Peterburgh, will accompany the authors in their journey to the Ural Mountains this summer.—*March* 26.

LXXIV. *Intelligence and Miscellaneous Articles.*

MR. BRAYLEY'S LECTURES ON IGNEOUS METEORS AND METEORITES.

THE following is the Syllabus of a Course of Lectures on Igneous Meteors and Meteorites which I am about to deliver at the London Institution, as announced in our last Number, p. 414, as it has been

* Similar Jurassic beds had been previously observed by M. Strajeske in the N. Ural, Lat. 64° north, and their fossils are described by M. Leopold de Buch in his recent work, 'Beitrage zur bestimmung der Gebirgs-formationen in Russ-land.'

printed for distribution among the Proprietors of the Institution and other attendants on the Lectures. It contains indications of certain views on the subjects of the Course, to which I may hereafter have occasion to refer.

LECTURE I. Thursday, November 11, 1841.—Introduction: connexion of the subject with that of the course on Igneous Geology delivered last year; the operation of *Heat* on certain kinds of matter, whether in Meteors or in the crust of the earth, produces crystalline mineral aggregates or *rocks*. History of our knowledge respecting Meteorites or Meteoric rocks. The fall from the heavens of stones and blocks of iron recorded by the historians and naturalists of Classical Antiquity and the Middle Ages. The statements of those and subsequent writers disbelieved in more recent times; but their truth demonstrated, and the descent of such bodies from Igneous Meteors established as a fact in science early in the present century. Fall of a large stone in Yorkshire in 1795—of a shower of stones from a Meteor, near Benares, in India, in 1798—of a shower of several thousand stones at L'Aigle, in Normandy, in 1803.

LECTURE II. Nov. 18.—Phænomena of those Meteors which assume the form of Balls of Fire, and are thence called *Igneous Meteors* and *Fire-Balls*. The Meteor which passed over Great Britain in 1783 taken as an example. Figure and aspect of Igneous Meteors in general, with pictorial illustrations. Their light more intense than that from any other source except the Sun: their velocity equal to that of the planets and relatively much greater. Their height above the Earth's surface—their explosions—the luminous tracks they leave in the sky. Some throw down red-hot stones, others blocks of iron. Fall of a shower of stones from a great Meteor which appeared in North America in 1807. More recent instances in India, Russia, Germany, the British Islands, the South Sea Islands, Brazil, and Southern Africa. The most recent authenticated instance—that of a large stone which fell at Château-Renard, near Orleans, on the 12th of June, 1841. Other Phænomena attending the fall of Meteorites described and illustrated.

LECTURE III. Nov. 25.—The nature of Meteoric Stones, their mineralogical characters and constitution, and chemical composition, illustrated by specimens. The immense blocks of metallic iron, some many tons in weight, found on the surface of the earth in various countries, especially in Africa and America, all cast down by Meteors. Observed fall of such a block, in Hungary, in 1751. The peculiar nature of Meteoric Iron illustrated. Meteoric stones and iron form one class or series of bodies, collectively termed METEORITES, or Meteoric rocks. Some meteorites mineralogically identical with certain volcanic rocks: these two classes of rocks still perfectly distinct: both of igneous origin, but the production of the one attended by *meteoric* phænomena in the atmosphere or the planetary spaces, and of the other by *volcanic* phænomena within the Earth's crust. Final illustrations of the peculiar characters of Meteorites.

LECTURE IV. Dec. 2.—The problem of the origin of Meteorites

discussed. Examination of some of the hypotheses by which its solution has been attempted, and in which meteorites have been supposed to originate in the Earth or its atmosphere—the volcanos of the Moon—other planets—the Sun—the planetary spaces—more distant parts of the Universe—to be themselves minute planets or satellites, fragments of planets which have been destroyed, or matter preparing for the formation of new worlds. Relations of this subject to that of the identity or similarity of Fire-Balls and the meteors called *Shooting Stars*: recent simultaneous appearance at distant parts of the globe of showers of shooting stars—their alleged periodicity. Manifest connexion between the physical and chemical characters of Meteorites and the phænomena which attend their fall; the former illustrated from the molecular constitution of the stony lavas, in the flowing state: resulting inferences as to the intimate nature of Igneous Meteors. Connexion of this branch of the subject with many important objects of inquiry in Meteorology, Astronomy, and Geology. Conclusion.

The illustration of the characters of meteorites from the molecular constitution of the stony lavas, in the flowing state, to be alluded to in the concluding lecture, arises from the application to the subject of Meteorites, of certain considerations derived from Mr. Scrope's inductions respecting the constitution of lava and the nature of its apparent fluidity. I have, for the last four years, been in the habit of explaining and supporting those inductions, in lectures on Igneous Geology. The following is the manner in which I have most recently treated this subject, as stated in the Syllabus of some lectures on Igneous Geology delivered at Clapham in June last, in sequence of others given by Dr. Mantell on those departments of geological and palæontological science to which his own discoveries and researches have so greatly contributed.

"Currents of *Lava*—their particular Nature and History—the Minerals of which they are constituted.—Stony Lavas—Graystone—Trachyte—Basalt.—Glassy Lavas—Obsidian—Pitchstone—Pearlstone—Pumice.—Alternate production of Stone and Glass from the same Materials, both in Volcanic and in Artificial Operations.—Circumstances under which this takes place.—Difference between the Molecular Constitution of the Stony Lavas and that of the Glassy, in the flowing state.—Important theoretical bearings of this branch of the subject." E. W. B.

London Institution, Nov. 8, 1841.

ACTION OF SULPHURIC ACID ON PROTOSULPHATE OF IRON.

M. Callond states, that when cold concentrated sulphuric acid is added to protosulphate of iron, the acid becomes of a red colour, which is more or less intense. When the sulphuric acid, on account of its deficient density, or any organic matter which it may contain, does not produce the effect, a drop or two of nitric acid rapidly produces it, without the application of heat.—*Journal de Chimie Médicale*, June, 1841.

BOOKS.

An Experimental Inquiry concerning the relative Power of, and useful Effect produced by, the Cornish and Boulton and Watt Pumping Engines, and Cylindrical and Waggon Head Boilers. By Thomas Wicksteed, M.I.C.E.

The Elements of Mechanics; designed for the Use of Students in the University. By James Wood, D.D., late Master of St. John's College, Cambridge, and Dean of Ely. A new Edition, revised and re-arranged, with considerable alterations and additions, by J. C. Snowball, M.A., Fellow of St. John's College, Cambridge.

Illustrations of Arts and Manufactures; being a selection from a Series of Papers read before the Society for the Encouragement of Arts, Manufactures, and Commerce. By Arthur Aikin, F.L.S., F.G.S., &c.

The Elements of Descriptive Geometry. By the Rev. Professor Hall, King's College, London. Eighty Illustrations.

Examples of the Processes of the Differential and Integral Calculus. Collected by D. F. Gregory, M.A., Fellow of Trinity College, Cambridge.

Cruveilhier's Anatomy, Vol. I. translated by Dr. Maddon; with Notes by Prof. Sharpey, Professor of Anatomy and Physiology in University College.

On the Remote Cause of Epidemic Diseases. By John Parkin.

Transactions of the Manchester Geological Society, vol. i.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1841.

Chiswick.—October 1. Thick haze: cloudy. 2. Very fine. 3. Cloudy: rain. 4. Fine: rain at night. 5. Cloudy: rain. 6. Fine: cloudy: clear at night. 7. Fine: rain. 8. Overcast: slight rain. 9. Cloudy: rain. 10. Overcast: rain. 11. Very fine: rain. 12. Heavy showers. 13. Fine: stormy with rain at night. 14. Boisterous. 15. Heavy rain. 16. Rain: densely clouded: rain at night. 17. Overcast: stormy. 18. Clear and windy: rain at night. 19. Cloudy: fine. 21. Clear: frosty at night. 22, 23. Overcast: rain at night. 24. Cloudy. 25. Overcast: foggy. 26. Foggy: stormy with rain at night. 27. Boisterous with very heavy rain. 28—30. Rain. There were only four days in the month on which rain did not fall, leaving a proportion of wet days perhaps unequalled in the neighbourhood of London.

Boston.—Oct. 1. Fine: rain yesterday P.M. 2. Fine. 3, 4. Fine: rain P.M. 5. Rain: rain early A.M. 6. Cloudy: rain early A.M. 7. Fine. 8. Fine: rain P.M. 9. Fine. 10. Cloudy: rain P.M. 11. Fine. 12. Cloudy: rain early A.M. 13. Fine: rain P.M. 14. Cloudy. 15. Cloudy: rain early A.M. 16. Rain: rain early A.M. 17. Rain: rain early A.M.: stormy night. 18. Stormy. 19. Cloudy: rain early A.M. 20. Stormy: rain at night. 21, 22. Fine. 23. Rain. 24. Cloudy: rain early A.M. 25, 26. Fine. 27. Cloudy: rain P.M. 28, 29. Stormy: rain early A.M. 30. Stormy: rain early A.M.: rain P.M. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Oct. 1. Rain for an hour. 2. Fine clear sunshine. 3. Dull, cold and rainy. 4. Fair but dull. 5. Showery forenoon. 6. Wet nearly all day. 7, 8. Dropping day. 9. Fair but threatening. 10. Wet all day. 11. Wet afternoon. 12. Heavy rain A.M.: cleared up. 13, 14. Heavy rain P.M. 15—17. Showers. 18. Fair throughout. 19. Showery afternoon: frost A.M. 20. Very heavy rain. 21. Fair and frosty. 22. Frost A.M.: cloudy and moist P.M. 23. Showers. 24. Fair but cloudy. 25. Fair and clear. 26—28. Fair and bracing. 29. Fair and bracing: frost. 30. Fair A.M., but showers P.M. 31. Slight showers.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Oct.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dum-fries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
1.	29·478	29·598	29·424	28·94	29·28	29·48	55·7	64·3	53·6	64	43	51	55	49	ENE.	N.	calm	SW.	·100	·20	·14	...	56
2.	29·812	29·773	29·730	29·26	29·63	29·77	53·3	61·0	49·0	63	42	52	52	35	SW.	W.	calm	SW.	·313	...	...	...	53
3.	29·832	29·792	29·745	29·30	29·83	29·75	52·5	60·8	47·7	63	51	49	53	34	N.	NW.	calm	SE.	...	·13	...	...	57
4.	29·718	29·703	29·329	29·17	29·72	29·41	54·9	58·4	51·3	61	49	55·5	62½	42	N.	NE.	calm	E.	·125	·36	·09	...	53
5.	29·160	29·124	29·888	28·65	29·12	28·82	53·2	60·5	51·7	62	43	53·5	53	46	S.	S.	calm	NE.	·272	·22	·63	...	54
6.	28·848	29·925	29·808	28·33	28·78	28·79	52·3	58·8	49·0	61	41	52	50	44	SSE.	W.	calm	ENE.	·205	·04	·09	...	53
☾ 7.	29·040	29·038	29·991	28·45	28·85	28·90	53·3	59·7	48·6	61	45	49	54	47	S.	SW.	calm	NNE.	·063	·04	...	0·82	52
8.	29·200	29·224	29·027	28·69	29·13	29·38	52·7	59·3	48·8	57	45	50	54½	47	S.	SW.	calm	NNE.	·012	·03	...	...	51
9.	29·778	29·872	29·712	29·18	29·54	29·61	50·7	58·2	48·5	57	42	51	54½	44	WSW.	W.	NW.	W.	·027	·03	·11	...	50
10.	29·878	29·835	29·612	29·35	29·51	29·20	54·3	57·2	48·4	59	50	48·5	55	47	S.	S.	calm	SE.	...	·21	...	...	49
11.	29·660	29·596	29·441	28·99	29·28	29·11	53·8	58·7	52·7	63	45	55	55	47	SSW.	SW.	calm	S.	·150	·34	·08	...	52
12.	29·254	29·596	29·202	28·70	29·15	29·38	52·7	60·0	49·3	57	42	51	53	44	S.	W.	SW.	SSE.	·352	·24	·02	...	52
13.	29·922	29·973	29·859	29·30	29·72	29·47	49·7	55·7	46·0	58	50	48	54	40½	W.	NW.	NW.	W.	·186	·04	...	...	47
● 14.	29·860	29·821	29·698	29·16	29·42	29·25	58·3	59·8	49·8	64	53	57	57	49½	S.	SW.	calm	SSW.	...	·02	·14	2·50	53
15.	29·550	29·799	29·492	28·84	29·24	29·53	56·4	63·7	54·2	60	41	55	53	47	SW.	NW.	W.	W.	·022	·50	·04	...	53
16.	29·474	29·520	29·349	28·95	29·13	29·40	51·8	60·2	46·0	58	42	45	52½	40	S.	W.	calm	NW.	·441	·04	·39	...	50
17.	29·516	29·519	29·409	28·89	29·07	28·88	54·7	58·8	47·8	63	48	47	53½	36	SW var.	W.	calm	SSW.	·033	·01	·11	...	51
18.	29·704	29·829	29·591	28·96	29·48	29·62	52·5	62·6	50·5	58	43	48·5	53	40	W.	W.	NW.	NNW.	...	·42	·03	...	49
19.	29·628	29·962	29·524	29·12	29·57	29·55	46·3	56·3	47·0	51	31	44	52	30	W.	NW.	NW.	SW.	·427	...	·30	...	46
20.	29·926	29·911	29·611	29·35	29·40	29·07	47·8	51·7	43·2	56	37	44	52½	43	S.	SW.	SW.	SW.	...	·04	...	...	44
21.	29·896	30·162	29·818	29·32	29·63	29·87	42·7	55·0	40·7	50	26	39	44½	31	W.	W.	W.	WNW.	·019	...	·12	1·60	42
☽ 22.	30·170	30·124	29·861	29·74	29·86	29·60	41·8	49·0	37·7	57	38	36	46	30½	S.	SE.	calm	ESE.	...	·01	...	...	36
23.	29·456	29·449	29·034	29·06	29·15	28·88	53·8	55·5	42·0	57	45	46	51	42	S.	S.	SE.	E.	...	·21	·01	...	45
24.	28·944	29·964	29·924	28·37	29·73	28·80	49·3	58·3	48·6	56	31	48	50	40	S var.	W.	W.	NNW.	·250	...	·06	...	47
25.	29·098	29·234	29·061	28·59	29·00	29·25	44·2	55·6	44·0	54	35	43	47	41	S.	SW.	calm	N.	...	·01	...	...	46
26.	29·352	29·460	29·318	28·96	29·45	29·63	39·8	52·8	37·2	51	42	45	46	37	W.	NE.	calm	N.	...	·11	...	...	39
27.	29·460	29·465	29·406	29·13	29·71	29·73	47·5	51·2	40·0	49	45	48	46½	36½	N.	NE.	NE.	NE.	·066	·92	...	...	45
28.	22·642	29·820	29·601	29·30	29·83	29·92	46·8	50·2	47·2	48	43	48·5	49	37	N.	NE.	NE.	NE.	·930	·10	·24	0·18	46
29.	29·848	29·818	29·766	29·45	30·00	30·03	46·7	49·6	44·7	50	45	47	49	33	N.	NE.	NE.	NE.	·069	·09	·05	...	45
○ 30.	29·784	29·776	29·721	29·45	30·02	29·85	45·7	50·5	46·0	49	45	47·5	46	37½	N.	NE.	NE.	NE.	·058	·06	·02	...	44
31.	29·838	29·877	29·798	29·36	29·70	29·71	49·3	50·5	46·0	53	44	51	50	41½	N.	SE.	NE.	NE.	·122	·19	·30	...	47
Mean.	29·572	29·695	29·572	29·0	29·417	29·407	50·5	56·9	47·0	57·10	42·64	48·5	51·7	40·6					Sum. 4·142	4·61	2·97	5·10	Mean. 49

THE

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SUPPLEMENT TO VOL. XIX. THIRD SERIES.

LXXV. *On an extraordinary Magnetic Disturbance recently observed at Greenwich. By* G. B. Airy, *Esq., M.A., F.R.S., Astronomer Royal**.

ON the 25th of September of the present year, a most extraordinary disturbance of the magnetic instruments was observed at the Magnetic Observatory attached to the Royal Observatory of Greenwich. The disturbed state of all the instruments attracted the attention of Mr. Glaisher (chief assistant in the magnetic department), at an early hour of that day, and he immediately commenced a series of observations on all the instruments, at short intervals. After a time, the disturbance became so small that the extraordinary observations were discontinued; but it again increased, and observations were again immediately made with all the instruments, and repeated as rapidly as it was possible for one observer to do so. But for the promptitude and judgement displayed by Mr. Glaisher on this occasion, the record of this disturbance, unprecedented in this magnetic latitude, would probably have been lost. The following statement will give an idea of the magnitude of the disturbance:—that within eight minutes of time, the declination needle changed its position *more than* $2\frac{1}{4}^\circ$ (having passed in both directions the range of the observing telescope, which includes that angle); that the vertical force was increased by *more than* 1-40th of its whole value, the instrument having then reached the extremity of its range; and that the horizontal force was increased about 1-30th of its whole value.

The following more detailed account is taken principally from the abstract furnished by Mr. Glaisher. It is to be remarked, that the time throughout is Göttingen mean solar time, civil reckoning: that the readings of the theodolite for

* From a paper printed for private circulation, and kindly communicated by the Author.

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the declination needle increase as the north end of the needle moves towards the east, and that the reading for the astronomical meridian is 269° 51′ 45″ nearly; that the increasing readings of the vertical-force magnetometer imply increasing vertical force, one division being equal to 0·000471 of the whole vertical force; and that the increasing readings of the horizontal-force magnetometer imply increasing horizontal force, one division being equal to 0·002214 of the whole horizontal force.

Early in the morning of Sept. 25, the needles were in an agitated state. During the appearance of an aurora, additional observations were taken; and the declination needle in less than three hours traversed an arc of 34′. After this the needles were in a tolerably quiet state, and extraordinary observations were discontinued.

The observation at 10^h a.m. showing a change of 17′ of arc from the previous reading, at 8^h a.m., extra observations were again resumed, and continued till 11^h a.m.; nothing remarkable appearing during this time, they were again discontinued.

At 2^h p.m., Göttingen mean time, it was evident that all the needles were affected by some unusual cause of disturbance, and incessant observations were at once commenced, the instruments being observed successively, as quickly as one observer could take them. At 3^h 53^m Mr. Hind joined, and subsequently Messrs. Dunkin and Paul. From this time to the discontinuance of the observations two persons were constantly engaged, one taking the observations with the vertical-force magnetometer, the other, those of the declination needle and of the horizontal-force magnetometer. About 2^h 40^m p.m., the motions of the needles became very peculiar; the vibrations of the declination needle and of the vertical-force magnetometer being quite destroyed; and those of the horizontal-force magnetometer being reduced to a continuous succession of jerks or starts. It will perhaps contribute to clearness if the motions of the three instruments are now separately described.

Declination Needle.—Every change in this instrument from one position to another was by sudden impulses; after each of these it was stationary, for intervals varying from 5^s to 20^s; then it was forced again to another position, and was again stationary without the slightest motion; and so on successively.

At 3^h 30^m the changes from one position to another became very decided, but the motions still partook of the same character; absolute rest and jerking motion followed alternately:

in 2^s or 3^s the needle frequently moved through many minutes of arc, and then suddenly became stationary again.

At $3^h\ 36^m\ 20^s$ p.m., a bold sweep carried the north end of the needle towards the west 56′ of arc, in one minute of time; at $3^h\ 37^m\ 20^s$ the theodolite read 245° 22′ −; and for two minutes after this time the cross carried by the needle was out of range of the telescope *, being in a position, in which, if the telescope had been exactly directed to it, the theodolite would have read less than 245° 22′. At $3^h\ 40^m\ 20^s$ p.m., Göttingen mean time, the needle had moved back 25′; at $3^h\ 43^m\ 50^s$ the theodolite read 246° 19′; at $3^h\ 44^m\ 20^s$ the needle had moved so far (north end towards the east), that the theodolite reading was 247° 38′ +, and the cross carried by the needle was out of range of the telescope on the opposite side.

This extraordinary magnetic stride of more than $2\frac{1}{4}$°, was traversed in about 8^m, the extremes being only separated by that interval of time.

At both extremities the needle was without swing, all momentum from such a wide sweep being quite destroyed.

After remaining at this place for more than two minutes, the needle slowly returned, and at $3^h\ 48^m\ 54^s$ the theodolite read 246° 27′; at $3^h\ 53^m\ 21^s$ it read 246° 37′. Within 5^s after this, another bold sweep carried the cross out of the range of the telescope; when found, at $3^h\ 54^m\ 40^s$, the theodolite read 245° 31′ ±; instantaneously it rushed back, with a violently agitated motion, across and out of the field on the other side; the circle-reading, at $3^h\ 55^m\ 0^s$, was 246° 43′ ±, and therefore this arc of 1° 12′ ± was passed over in 20^s of time.

This was the last very great excursion; the needle was incessantly watched for 9^h longer, during which time it was much agitated. The most remarkable changes are the following:—

	h	m	s			°	′
At	4	25	36	p.m., Göttingen mean time,	theodolite read	246	8
		31	36	...	...		34
		49	36	...	...		8
		55	36	...	...		21
	5	46	37	...	...		10

* This arose from the great inclination of the axis of the collimator (carried by the magnet) to the magnetic meridian; in consequence of which, the pencil of rays passing through the object-glass of the collimator was thrown entirely on one side of the object-glass of the theodolite. In ordinary cases, the change of angle has been sufficiently slow to allow the observer to shift the suspension of the magnet, but the rapid changes on Sept. 25 did not allow the observer to leave his place. Observations with the mirror in Gauss's manner are free from this inconvenience.

h m s			° ′
At 5 50 37 p.m., Göttingen mean time, theodolite read			246 30
6 0 30	...	...	8
33 37	...	...	26
46 36	...	...	32
7 16 38	...	...	25
31 38	...	...	46
56 38	...	...	37
8 53 38	...	...	28
9 18 38	...	...	43
38 38	...	...	51
10 10 39	...	...	53
41 39	...	...	39
46 39	...	...	46
11 45 40	...	...	30
12 52 40	...	...	47

It is to be remembered, that increasing readings denote that the north end of the needle moves towards the east.

Vertical-Force Magnetometer.—The vertical-force magnetometer was as much affected as the declination instrument. The disturbances of its vibrations were very similar, except that it was frequently much longer in the stationary state, its times of rest varying from 5^s to 2^m; at the end of each of these it jumped on, then was again still, and so on.

This instrument had been much affected early in the morning; the extent of its excursions being between $37^d{\cdot}9$, and $44^d{\cdot}1$ on the scale. The following are scale-readings, commencing with noon:—

h m s			d
At 0 0 0 (noon), Göttingen mean time, scale-reading			44·0
1 47 30 p.m.	...	...	47·8
57 30	...	...	48·1
2 7 30	...	...	50·0
3 15 21	...	...	55·0
16 21	...	...	55·9
21 26	...	...	57·9
22 21	...	...	58·3
22 51	...	...	59·0
23 21	...	...	60·3
23 51	...	...	60·3
24 21	...	...	60·4
26 21	...	...	63·0
27 51	...	...	63·2
31 21	...	...	67·0
32 21	...	...	69·5
32 51	...	...	71·2
34 21	...	...	81·3
34 51	...	...	95·0

At $3^h\ 35^m\ 21^s$, the marked end of the needle (which is towards the east, the needle being transverse to the magnetic meridian) was resting on the eastern Y, or apparatus intended for guarding the needle, dipping as much as the frame permitted it, and held there apparently by a great power; in this situation it remained for ten minutes, till $3^h\ 45^m$.

	h	m	s				d
	At 3	45	42	p.m., Göttingen mean time, scale read			73·1
		46	23		...	...	68·9
		50	31		...	...	63·4
		57	38		...	...	57·7
		58	58		...	...	55·6
	4	13	36		...	...	62·9
		29	26		...	...	66·9
	5	21	37		...	...	55·6
		44	37		...	...	60·6
		55	37		...	...	53·2
	6	19	7		...	...	68·3
	7	17	8		...	...	53·8
		35	8		...	...	50·3
	9	22	9		...	...	45·6
Sept. 25.	11	0	39		...	...	40·9
Sept. 26.	0	58	25	a.m.	...	...	33·7
	1	3	25		...	...	36·1

About $6^h\ 19^m$ p.m., the needle assumed a steadier appearance, having a motion something like vibration, but exceedingly irregular.

Horizontal-Force Magnetometer. — The horizontal-force magnetometer was not less affected than the other two instruments. The effect on its vibrations was however peculiar to itself: in the midst of its swings it was suddenly checked, and held in that place for a second or two, then hurried backwards, again suddenly stopped, and then forced back in the contrary direction; these checks were sometimes at 5^s, sometimes at 10^s, after arriving at its limit. They were most striking. From experience with this instrument we are accustomed to look for an almost imperceptible motion at the extremities of the vibrations, then to see it move slowly in the opposite direction, quickly in the centre of swing, slowly towards the end, then return, and so on; but to see it, almost in the same instant, moving rapidly and absolutely motionless, was very striking and strange to the observers.

This instrument was less affected in the early part of the day than either of the others. About noon it showed signs of being under the influence of some disturbing cause: the scale then read $55^d{\cdot}8$; at $1^h\ 50^m$ the reading increased to $58^d{\cdot}6$; at $2^h\ 0^m$ to $59^d{\cdot}3$; at $2^h\ 10^m$ to $60^d{\cdot}1$; it then oscillated

between 59^d and 61^d, till $3^h\ 11^m$, the scale then reading $59^d{\cdot}7$. The disturbances now became as surprising as those with the other needles; a steady and rapid increase of scale-reading took place, with a strange jerking motion.

	h	m	s			d
At	3	14	48	p.m., Göttingen mean time,	the scale read	61·4
		21	3	...	...	61·2
		27	26	...	...	64·2
		28	6	...	...	64·4
		28	46	...	...	64·8
		38	16	...	...	73·6
		38	56	...	...	73·2

Then there was a remarkable start to $78^d{\cdot}0$; and here it remained stationary from $3^h\ 39^m$ to $3^h\ 43^m$, when it started back.

	h	m	s			d
At	3	47	41	p.m., Göttingen mean time,	the scale read	69·9
		56	31	...	...	65·6
	4	0	51	...	...	63·7
		8	55	...	...	63·1
		15	11	...	...	64·6
		47	41	...	...	61·4

Then the reading increased a little, and then gradually decreased, till $5^h\ 53^m\ 12^s$ p.m., Göttingen mean time, when it read $57^d{\cdot}8$; it then increased to $6^h\ 16^m\ 12^s$, when the scale read $62^d{\cdot}1$.

It then oscillated between 58^d and 60^d, until $7^h\ 25^m\ 13^s$, when a sharp diminution to $53^d{\cdot}9$ took place. The readings alternately increased and decreased for the next six hours, oscillating all the time between 53^d and 57^d.

From the preceding account it will be seen, that the magnetometers were more affected on this day than ever before, and that the great disturbance affected the three instruments at the same time.

The most abrupt and violent fluctuations of the meridian needle occurred between $3^h\ 36^m$ and $3^h\ 46^m$. At $3^h\ 35^m\ 21^s$ the vertical-force magnet went beyond the scale; at $3^h\ 45^m$ it again came on. The horizontal-force needle began its great deflexion before $3^h\ 38^m\ 16^s$; it was over at $3^h\ 48^m$; and, probably, had there been an observer at each instrument, it would have been found that the effects on all were simultaneous. Such was Mr. Glaisher's impression while observing; but (as he remarks) the startling and bewildering effect of so great changes was such, that all that he could do was to make such observations as seemed practicable, with a view of discovering the *extent* of such extraordinary disturbances.

After this great disturbance the irregularities were still very great; the horizontal-force and the vertical-force magnetometers partook of similar disturbances throughout. With a few trifling exceptions, an increase of force in one was accompanied with an increase of force in the other, and so also with respect to the decrease of force.

The declination magnet has certainly, however, one great deflexion, in which the others have no share; that which occurred between $3^h\ 54^m\ 40^s$ and $3^h\ 55^m\ 0^s$, where an arc of $1°\ 12'$ was described in 20^s. Nothing similar, at this time, is shown by either of the other needles.

In addition to the observations contained in this account, ample and abundant observations were taken to obtain every variation of the vertical force, and also observations of the declination and horizontal-force magnetometer, sufficient to show every great change and nearly all the small ones, until Sept. $26^d\ 1^h$ a.m., when the observations were discontinued.

At $4^h\ 40^m$ an observation of the dip was taken; giving a result greater than ordinary.

The day (Sept. 25) was cloudy throughout: about 9^h p.m., a few bright streamers were seen through the clouds, then nothing more till 11^h p.m., when an auroral arch, about 24° high, was visible for a short time.

It is desirable that accounts of corresponding observations of this disturbance should be collected. I invite observers who may have made observations on the same day, to send to me abstracts of their observations, or to communicate them, in the way which they think best, to the public, or to the persons who are interested in magnetical phænomena.

Royal Observatory, Greenwich, 1841, Oct. 26. G. B. Airy.

LXXVI. *Table of the successive Strengths of Pyroxylic Spirit, corresponding to its successive Specific Gravities, with some Introductory Observations.* By Andrew Ure, *M.D., F.R.S., &c.**

HAVING been professionally employed by an eminent manufacturing chemist, about eighteen months ago, in experimental researches upon the above spirit, the *holzgeist* of the Germans, I found it necessary to construct the following table, in order to ascertain the commercial value of the article at various densities. The principal use of wood-spirit, as extracted by distillation from pyrolignous acid, or from liquid pyrolignite of lime, is for dissolving shell-lac and sandarac into a varnish for stiffening the bodies of hats, and rendering them

* Communicated by the Chemical Society, having been read June 1, 1841.

impervious to water. Hats imbued with this varnish exhale in the hot apartments where the process is conducted the vapours of the wood-spirit very copiously, and thereby occasion a painful irritation to the eyes of the workmen. Some kinds of the spirit are much more injurious to the eyes and the health than others, even when they have all been rectified to apparently the same pitch of purity and strength by the same operations. One purpose of my researches was to discover the causes of these variations, which affect the comfort of the operatives, and another was to discover the causes of the variations in the solvent qualities of wood-spirit of the same strength by the hydrometer. Having hitherto but partially succeeded in the attainment of these two objects, I shall not occupy the time of the Society at present with an account of the experiments made with that view, but shall reserve them for a future communication.

The researches of Berzelius, Gmelin, Weidmann, Schweitzer, Kane, Liebig, Dumas, and Peligot, concur to prove that the ordinary wood-spirit of commerce, even in its most highly rectified state, is not like spirit of wine, merely an alcoholic liquor more or less diluted with water, but that it consists of different compounds mingled together, and very difficultly separable from each other. Wood-spirit, xylite, and mesite, are three of these liquid compounds usually associated in pyroxylic spirit. When the common wood naphtha of the druggist is distilled three or four times from pulverized unslaked quicklime, by the heat of a water-bath, the oily impurities and water are got rid of, and an anhydrous fluid is obtained which is not liable to become brown on exposure to light, like the ordinary wood naphtha, and which does not become turbid or milky when mixed with water. This purified spirit, however, still acts as painfully almost as the original cruder article, upon the eyes of the hatters, as I ascertained by trial. One mode of separating *wood-spirit* from xylite and mesite, is founded upon the property possessed by wood-spirit, of forming a compound with chloride of calcium not decomposable at the heat of boiling water, while similar compounds with xylite and mesite are decomposable at that temperature. I did not find that pyroxylic spirit was essentially improved as to its employment in the arts, by being rectified by distillation from its combination with chloride of calcium.

Methol is the name which has been given to the oil formed by the action of sulphuric acid upon wood-spirit, xylite, and mesite; and I believe the same oil is generated by the simple combustion of pyroxylic spirit; for I have observed that when

the pyroxylic spirit, which has been treated with both quicklime and chloride of calcium, is burned in a platinum capsule till fully one-half be consumed, the residuum becomes oily and opalescent.

The spirit used for the construction of the following table was purified by distillation from pulverized quicklime, and was drawn over with the heat of a water-bath, at such a temperature that its specific gravity at 60° was 0·8136. When the specific gravity becomes 0·847 by the dissipation of the lighter spirit, the boiling point is 145° Fahr. I believe that a useful criterion of the nature of pyroxylic spirit would be obtained by comparing its boiling temperatures at different degrees of density. To this point I shall also direct my further investigations.

The temperature of the pyroxylic spirit when the specific gravities were taken, was exactly 60° Fahr.

Spec. Grav.	Spirit per cent.	Over proof of Excise scale.	Spec. Grav.	Spirit per cent.	Over proof of Excise scale.
0·8136	100·00				
0·8216	98·00	64·10	0·9032	68·50	13·1
0·8256	96·11	61·10	0·9060	67·56	11·4
0·8320	94·34	58·00	0·9070 ?	66·66	9·3
0·8384	92·22	55·50	0·9116 x	65·00	7·10
0·8418	90·90	52·50	0·9154	63·30	4·20
0·8470	89·30	49·70	0·9184	61·73	2·10
0·8514	87·72	47·40			Under proof.
0·8564	86·20	44·60	0·9218	60·24	0·6
0·8596	84·75	42·20	0·9242	58·82	2·5
0·8642	83·33	39·90	0·9266	57·73	4·0
0·8674	82·00	37·10	0·9296	56·18	7·00
0·8712	80·64	35·00	0·9344	53·70	11·00
0·8742	79·36	32·70	0·9386	51·54	15·30
0·8784	78·13	30·00	0·9414	50·00	17·80
0·8820	77·00	27·90	0·9448	47·62	20·80
0·8842	75·76	26·00	0·9484	46·00	25·10
0·8876	74·63	24·30	0·9518	43·48	28·80
0·8918	73·53	22·20	0·9540	41·66	31·90
0·8930	72·46	20·60	0·9564	40·00	34·20
0·8950	71·43	18·30	0·9584	38·46	35·60
0·8984	70·42	16·60	0·9600	37·11	38·1
0·9008	69·44	15·3	0·9620	55·71	40·6

LXXVII. *On the Ferrocyanides. By* [*the late*] R. Corbett Campbell. *Communicated by Dr.* Clark*.

IT is well known that the yellow prussiate of potash, heated by itself in close vessels, is decomposed into cyanide of potassium, carburet of iron, and nitrogen gas. I have observed

* Communicated to this Journal by the Chemical Society, having been read June 1, 1841.

that, heated in contact with the air, the products of the decomposition are very different. The cyanide of potassium takes oxygen from the air, and is thereby converted into cyanate of potash, while the cyanide of iron is decomposed, converting the iron into an oxide. The absorption of oxygen is caused by the presence of the cyanide of iron, for cyanide of potassium, heated by itself in contact with the air, does not become changed into cyanate of potash. I believe that a process for cyanate of potash may be founded on the above observation, preferable to the common one with oxide of manganese; for it has been proved that this oxide is not essential in the process, and it not unfrequently happens that a large quantity of the cyanide of potassium is converted into carbonate of potash. Sometimes also a little manganese dissolves along with the cyanate.

For the preparation of this salt, then, without the use of manganese, the powdered and dried prussiate of potash is heated almost to redness, in a flat iron vessel, with constant stirring of the melted mass. Some ammonia is evolved, which results from the action of the moisture of the air, for the substance itself contains no hydrogen. The melted mass should be taken out with an iron spatula, allowed to cool, reduced to powder, and again fused; because the melting cyanate of potash is apt to protect little bits of the yellow prussiate from the action of the air. If the heating be properly conducted, not a particle of cyanide of potassium will be formed. The cyanate of potash is dissolved out with hot alcohol filtered and crystallized. The undecomposed prussiate of potash remains undissolved.

The double salt of ferroprussiate of potash and lime acts under heat exactly like the ferroprussiate of potash itself. Well dried and set on fire, it continues to burn, until the alkaline and earthy cyanides are converted into cyanates, and the iron into oxide. The reason that the double salt continues to burn is to be found in the porous state of the mass, which offers no obstacle to the free access of air; whereas the yellow prussiate alone fuses, and prevents the progress of the combustion.

The ferrocyanide of zinc, which always contains some ferroprussiate of potash in chemical combination, likewise continues to burn, and affords cyanate of potash and the oxides of the metals.

When the double salt of potash and lime is heated in the air as above mentioned, and then dissolved in water, the solution possesses the remarkable property of becoming pink in the sun's rays, and again becoming colourless in darkness.

Neither cyanate of lime nor cyanate of potash, together or singly, or mixed with prussiate of potash, show this reaction. It is hence probable that this solution owes the above-mentioned property to the admixture of some foreign substance, present probably in a very small quantity. All attempts to isolate any such substance were fruitless.

As the shade of colour in the above solution is exactly that of a solution of permanganate of potash, the solution and the substances used were tested for manganese. They contained none. The reactions of this solution too stand in contradiction with some of those of the permanganates. The presence of ferrocyanide of potassium is essential to the production of the pink in the sun's rays, but the action of the same salt on permanganate of potash is first to reduce it to the green manganate, and by further addition of the prussiate to oxide of manganese. As the prussiate of potash of commerce often contains traces of sulphocyanide of potassium, the experiments were repeated with prussiate of potash, that had been washed with hot alcohol, and by this means all sulphocyanide removed; but no difference was observed in the result.

It is further clear, that this salt, even if it were present in the prussiate of potash, could take no part in the above-mentioned reaction, for it gives no precipitate with a lime salt, with or without the presence of prussiate of potash. An alkaline state of the liquid is essential to the production of the pink in the sun's rays; so is likewise the presence of a ferrocyanide. If a solution of nitrate of copper be added to a solution of the heated double salt, till all ferrocyanide be removed, and then the excess of copper precipitated by carbonate of potash, the solution will have lost entirely the property of becoming coloured in the sun's rays, but will recover it on the addition of a few drops of a solution of yellow prussiate of potash.

The air exercises no influence on these changes; they take place equally distinctly in closed vessels.

A temperature of 120° Fahr. destroys the colour, but on cooling and re-exposure to the sun's rays, the colour again appears. By evaporation in the rays of the sun, a pink salt is obtained. The presence of cyanate of potash is not essential to the production of the colour; if muriatic acid be added to the solution until all cyanic acid be destroyed or removed, and then supersaturated with alkali, the solution possesses the colouring property as strong as before. Another proof that cyanate of potash is not essential, is that the double salt of ferroprussiate of lime and potash heated in closed vessels, and

dissolved in water, shows likewise the same reaction, although containing no cyanate of potash.

I have already stated, that the investigation undertaken with the view of isolating the colouring substance in these experiments, was without success; nevertheless some observations were made which seem to stand in relation to this subject.

It has often been remarked, that, on adding an acid to the solution of the heated double salt, a minute quantity of a reddish powder is precipitated. This powder contains iron; it is decolorized by carbonate of potash, but the colour is not reproduced by exposure to the sun. The solution from which this powder has been precipitated, on being saturated with alkali, and exposed to the sun's rays, is found not to have lost the colouring property. I suspect that this red powder is the same as is observed to be precipitated from some specimens of commercial prussiate of potash, on the addition of strong acetic acid. Prussiate of potash is sometimes observed in commerce of a darker tint than usual, and it is this variety which frequently shows the above-mentioned reaction.

If a solution of cyanide of ammonium be added to a solution of acetate of copper, a compound of copper and cyanogen is precipitated, not analogous in its composition to the cyanide from which it is precipitated. The supernatant solution becomes pink-coloured for a few seconds. In order to see if this reaction result from the action of the free cyanogen on the cyanide of ammonium, a stream of this gas was sent through a solution of the salt. It became first yellow, and then red, but a different red from that mentioned in the reaction with the copper salt.

Professor Liebig pointed out to me another instance of a fugitive pink colour occurring in a cyanogen compound, which Prof. Gmelin of Heidelberg has mentioned in his System of Chemistry. When strong nitric acid is added to pounded yellow prussiate of potash, and slightly heated, much cyanogen gas is evolved; when the heat is not too strongly applied, neither prussic acid nor nitrous acid are remarked. The mass becomes black, and is quite soluble in water: if there be added to this solution, first, an excess of potash, and then some drops of sulphuret of potassium, the solution becomes beautifully pink-coloured, disappearing after the lapse of a few seconds.

It differs from the pink colour observed as occurring in the solution of the heated double salt when exposed to the sun, in its action to sulphuretted hydrogen. This gas, or an alkaline sulphuret, instantly destroys the colour resulting from the

action of light, while an alkaline sulphuret is essential to the production of the other. The pink colour of the solution to which sulphuret of potassium has been added, becomes soon purple, and afterwards deposits a blue substance, which long kept in the solution becomes white. This blue precipitate cannot be prussian blue, for it is produced in an alkaline solution, and is instantly decolorized by acids. It is possible that the above pink solution owes its colour to the same substance as is present in Gregory's solution of sulphuret of azote in caustic potash.

I have brought forward these different instances of coloration occurring in cyanogen compounds, because they seem to point out the existence of a yet unexamined class of prussiates.

On the Acid Cyanurate of Potash.

Mineral acids, as well as acetic acid, were found to have the power of converting the neutral cyanate of potash into the acid cyanurate. The essential circumstance in this process is that the solution of the cyanate of potash be concentrated. The process above described for making cyanate of potash by calcining in the air the yellow prussiate of potash, affords a ready means of making this salt easily, and in quantity. The roasted prussiate is digested with cold water, filtered, and muriatic acid, added to the precipitated salt, is dissolved in hot water, and crystallized by cooling.

The foregoing experiments were partly confirmed and partly originally performed in the laboratory at Giessen, in 1838.

This paper is the only scientific memorial of its amiable author, who died about two years ago, at an early age, but mature enough to have endeared him to many men of science, and to have indicated promise of scientific eminence. He will be most remembered by his friends for a marked rectitude of mind, combined with an uncommon ardour; evincing itself in the warmth of kindly feelings and in the enthusiasm of scientific pursuits.—T. C.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 489.]

June 17, 1841. (*Continued.*) "ON the Corpuscles of the Blood." Part III. By Martin Barry, M.D., F.R.SS. L. and E.*

After remarking that no clear conception has hitherto existed of the mode in which the floating corpuscles of the blood conduce to nou-

[* Abstracts of Parts I. and II. of this memoir will be found in Phil. Mag., S. 3. vol. xvii. p. 300, and vol. xviii. p. 310.—Edit.]

rishment, the author states that he has found every structure he has examined to arise out of corpuscles having the same appearance as the corpuscles of the blood. The following are the tissues which he has submitted to actual observation, and which have given the above result, namely, the cellular, the nervous, and the muscular; besides cartilage, the coats of blood-vessels, several membranes, the tables and cells of the epithelium, the pigmentum nigrum, the ciliary processes, the crystalline lens itself, and even the spermatozoon and the ovum.

The author then traces the nucleus of the blood-corpuscle into the pus-globule; showing that every stage in the transition presents a definite figure. The formation of the pus-globule out of the nucleus of the blood-corpuscle is referable to the same process, essentially, as that by means of which the germinal spot comes to fill the germinal vesicle in the ovum. This process, which, in a former memoir, he had traced in the corpuscles of the blood, he now shows to be universal, and nowhere more obvious than in the reproduction of the tables of the epithelium. The epithelium-cylinder seems to be constituted, not by coalescence of two objects previously single, as has been supposed, but by division of a previously single object. Certain objects, called by the author *primitive discs*, exhibit an inherent contractile power, both when isolated, and when forming parts of a larger object; an incipient epithelium-cylinder having been observed by him to revolve by this means. Molecular motions are sometimes discernible within corpuscles of the blood. The author has noticed young blood-corpuscles exhibiting motions, comparable to the molecular, and moving through a considerable space; and he has met with the nuclei of blood-corpuscles endowed with cilia, revolving, and performing locomotion. In his first paper on the Corpuscles of the Blood, he described certain instantaneous changes in form which he had observed in blood-corpuscles, and afterwards expressed his belief, that these changes were referable to contiguous cilia, although he had not been able to discern any such cilia. He now states that subsequent observation inclines him to think that these changes in form arise from some inherent power, distinct from the motions occasioned by cilia. The primitive disc, just mentioned, seems to correspond, in some instances, with the "cytoblast" of Schleiden. Thus the very young corpuscle of the blood is a mere disc; but the older corpuscle is a cell. The author minutely describes the mode of origin of the pigmentum nigrum; showing that it arises in a similar manner in the tail of the tadpole, and in the choroid coat of the eye. He had before described the Graafian vesicle as formed by the addition of a covering to the previously-existing ovisac: this covering, he afterwards stated, becomes the corpus luteum. He now confirms these observations, with the addition, that it is the blood-corpuscles entering into the formation of the covering of the ovisac, which give origin to the corpus luteum. The spermatozoon appears to be composed of a few coalesced discs. The fibres of the crystalline lens are not elongated cells, as supposed by Schwann; but coalesced cells, at first arranged in the same manner as beads in a necklace.

The author concludes with the following recapitulation:—1. The nucleus of the corpuscle of the blood admits of being traced into the pus-globule. 2. The various structures arise out of corpuscles having the same appearance, form, and size as corpuscles of the blood. 3. The corpuscles having this appearance, and giving origin to structures, are propagated by division of their nuclei. 4. The corpuscles of the blood, also, are propagated by division of their nuclei. 5. The minuteness of the young blood-corpuscles is sometimes extreme; and they are to be found in parts usually considered as not being permeable by red blood.

In a postscript, the author adds, that blood found in the heart immediately after death by bleeding, presents incessant alterations in the position of its corpuscles. Among these, when a single corpuscle is examined very attentively, it is seen to change its form; and the author is disposed to think it is this change of form that produces the alterations in position. The changes in form are slight, compared with those previously described by him as observed in blood elsewhere, and are not seen without close attention. The motions resemble those called molecular; and in the minutest corpuscles, which are mere points, nothing besides molecular motion can be discerned. It may be a question, the author thinks, whether molecular motion differs in its nature from the motion of the larger corpuscles just referred to. The division of the blood-corpuscles into corpuscles of minuter size, though apparent in blood from either side of the heart, has seemed more general in that from the left side; which, it is suggested, is perhaps deserving of notice in connexion with the subject of respiration.

"A new Theory of Physics, with its application to important phænomena hitherto considered as ultimate facts." By Thomas Exley, Esq., A.M.

The theory of the author is founded on the two following propositions, namely, that

1. Every atom of matter consists of an immense sphere of force, varying inversely as the square of the distance from the centre; this force being attractive at all distances, except in a small concentric sphere, in which it is repulsive.

2. Atoms differ from each other in their absolute forces, or in the extent of their spheres of repulsion, or in both these respects.

The author assumes that there are four classes of atoms, the *tenacious*, the *electric*, the *ethereal*, and the *aromatic*. The existence of the last-named class of atoms he infers from the phænomena of vegetation, the miasmata of marshes, the aroma of plants, various noxious effluvia, the disinfecting property of some bodies, and facts relating to animalcules, and their ova, &c. He regards the two propositions which constitute the great principles of his theory, as presenting, at once, a complete explication of the general attributes of matter and body, with the Newtonian laws of motion, not otherwise theoretically explicable.

After pursuing at some length his theoretical speculations, founded

on the above-mentioned propositions, the author concludes his paper with the following sentence:—

"The several partial theories of philosophers, as far as concerns the leading facts on which they are based, are contained in the simple principles here developed: thus, the theory of universal gravity is here carried out to its ultimatum; Newton and Boscovich's theories of alternate attractions and repulsions are derived from facts which depend on the alternate atmospheres, and neutral spaces of tenacious atoms; Sir Humphry Davy's theory of electrical energies, Dr. Dalton's atomic theory, and the theory of the diffusion of gases, Dr. Black's theory of latent heat, Gay-Lussac's theory of volumes, Newton's theory of light, or the theory of the emission of light, the undulatory theory, and very many others are here united in the most simple principles, which are, therefore, strongly recommended to the notice of philosophers."

"On the Organs of Reproduction, and on the Development of the Myriapoda." By George Newport, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author commences his paper by stating that great interest attaches to the study of the Myriapoda, from the already known fact that their mode of development, by an increase in the number of segments, is directly the reverse of that of true insects in which the development of the perfect individual is accompanied by an apparent diminution in the number of these parts. He remarks, that although the development of the Myriapoda has already been examined by several eminent naturalists, such as Degeer, Savi, Gervais, and Waga, some of the most important facts relating to it have, nevertheless, escaped their notice, and he proposes, therefore, to lay before the Society the result of his own investigations on this subject, and also his examinations of the organs of reproduction.

The paper is divided into four sections. In the first, the author describes the organs of reproduction, and shows that the parts described by Treviranus, both in the male and female Julus, are only the efferential ducts in the male, and the oviduct in the female; that in the former there are developed, from the sides of the efferential ducts, a large number of sacs, the structure of which he describes, and states his opinion that these are the proper secretory organs in the male, but remarks that he has not been able to follow out the organs to their fullest extent. In the female, he shows that the oviduct described by Treviranus is covered by an immense number of ovisacs, each secreting only a single ovum; that many hundreds of these exist around the duct, a large proportion of which never reach maturity, being retarded in their growth by the development of others immediately around them; and that the ova, when matured, are passed from the ovisacs into the duct, and are then all deposited at one time. He adverts especially to the remarkable condition of the female oviduct being a single organ, throughout the greater part of its extent, but having a double outlet; and shows its analogy in the internal portion of the organs to those of some in-

sects, and in its double outlet to the Crustacea and Arachnida. He also institutes a comparison between the structure of the male and female organs in this Myriapod, which, from their simplicity, admirably illustrate the uniformity of origin of these structures; more especially the analogy between the ovisacs in the female and the cæca in the male, and also their conformity in the absence, in the latter, of separate vesiculæ seminales, and, in the former, of spermatheca.

The second section is occupied by a short account of the structure of the ovum, in which the author observes the germinal vesicle and macula. He notices especially the presence of the yelk in the earliest stages of development, together with the vesicle and the membranes of the ovum at a later period, as showing in this low form of animal the conformity of structure and laws with those which prevail in the higher forms.

In the third section, the author speaks of the deposition of the ova, and of the habits of the species, as observed in specimens collected and preserved by him for that purpose. These habits he regards as particularly curious. The female excavates for herself a burrow, by digging with her mandibles in the soil, which she has previously moistened with a fluid, supplied, as the author believes, by her immense salivary glands. With this she forms a soft pellet, which she removes from the burrow with her mandibles and anterior legs; and thence, after being brought to the top of the hole, it is passed on to the next pair, and by these on to the next in succession, until it is entirely removed out of the way; after which, she deposits her eggs and closes the burrow with moistened clay. Great difficulty was experienced in preserving the eggs during the observations, from the circumstance that their shell is soft, and dries quickly when exposed to the air. To avoid this, the author had recourse to the plan of inclosing the eggs in a glass tube, filled with clay, and closed with a cork; the eggs being placed in a cell next to the glass.

The fourth section, which constitutes the most important part of the paper, gives the history of the evolution of the embryo. The process is divided by the author into different periods. After a few observations on the earlier changes of the egg, and the proof that they consist in an alteration in the size and appearance of the cells out of which the embryo is formed, he states his having observed that the egg bursts at the end of twenty-five days, by means of a fissure along the dorsal surface, as described by Savi and Waga; and that, in opposition to the remarks of Degeer, the young Julus, as first stated by Savi, is perfectly apodal. The author has also discovered a singular fact, entirely overlooked by all who have attended to the development of these animals, namely, that the young Julus at this time is still an embryo, and is completely inclosed in a shut sac, which terminates in a distinct *funis* at the extremity of the body, and in the proper *amnion*, or fœtal envelope of the animal. He finds, also, that the funis enters at the posterior penultimate segment of the dorsal surface of the body, and not at the dorsal surface of the thoracic region, as seen by Rathke in the Crustacea. The embryo, he says, is retained in connexion with the shell, between the two halves of it, for seven-

teen days, by means of the funis, which is continuous with a second, or external membrane, *the chorion*, which lines the interior of the shell. He states that the liberation of the embryo from the shell is not effected by any effort of its own, but by the expansive force of the growth of its body. He describes, also, another important fact which had been overlooked by previous observers, relating to the mode and place of origin of the new segment of the body in the Julidæ. The new segments are always produced in a *germinal membrane* immediately before the penultimate segment, which segment, with the anal one, remains permanent throughout the life of the animal. The production of the first set of new segments is commenced even before the animal has burst from the amnion. After leaving the amnion, the young Julus possesses six pair of legs, as stated by Savi and Waga; but the author remarks, in addition, that, notwithstanding this, it is still inclosed in another tunic, the proper skin of the embryo, beneath which new segments are being formed, and which begins to be detached before the embryo has left the amnion. He suggests whether this may not be the representative of the proper tunic of the germinal vesicle. After minutely describing the embryo, and showing that its body is still formed of cells, he states that four pairs of new legs are forming beneath this tunic, and that, on the twenty-sixth day, the young animal throws off this covering, and the legs are developed, and also the six new segments, to a further extent. The animal then takes food, the segments become developed to the same extent as the original ones, until the forty-seventh day, when it again changes its skin, new segments are again produced, and new legs to those segments last formed. In this way it passes through several changes, developing first segments and then legs.

One remarkable circumstance stated is, that the production of segments is *sextuple* in the Julidæ; but this does not hold in other genera, in some of which it is *quadruple*, and in others *double;* but these peculiarities appear in all cases to be characteristic of each distinct genus. In conclusion, he confirms the observation already made by M. Gervais, that the number of eyes is increased as the animal advances in its transformations. The author concludes by stating that he proposes continuing these observations on the Myriapoda at some future period.

The paper is accompanied by drawings of the parts described, and of the successive changes which take place during the development of the animal.

GEOLOGICAL SOCIETY.

[Continued from p. 500.]

April 7, 1841.—A paper was first read, entitled "A Notice on the Occurrence of Triassic Fishes in British Strata," by Sir Philip Grey Egerton, Bart., M.P., F.G.S.

Were the muschelkalk abstracted from the continental series of beds called the Trias, and the keuper made to rest on the bunter

sandstone, Sir Philip Egerton says, it would be difficult, if not impossible, to define the proper limits of these formations. The new red sandstone of England, the equivalent of the trias, presents this difficulty, every endeavour to find the muschelkalk having failed; and therefore geologists are compelled either to consider the keuper, the upper member of the trias, to be also wanting, or to be merged in the mass of alternating marls and sandstones comprising the new red series. Lithological structure, consequently, being of no value, palæontological evidence, the author says, becomes of great importance. The beautiful results arrived at by Mr. Owen respecting the Batrachian remains found near Warwick, tend, Sir Philip Egerton states, to render the existence of the keuper extremely probable, though a specific identification with the analogous fossils of the German keuper has not been ascertained. The only instances on record of muschelkalk fishes found in Great Britain, are scales from the Bone Bed at Aust Cliff, and referred by Professor Agassiz to *Gyrolepis Albertii* and *G. tenuistriatus*, common continental muschelkalk fishes. This bed it is well known occurs at the base of the lias, and rests conformably on the green and red marls of the new red sandstone. A thin stratum replete with remains of saurians and ichthyolites occupies a similar stratigraphical position near Axmouth; and Prof. Agassiz, during his visit to England in the autumn of 1840, identified in a series of specimens obtained by Miss Mary Anning, one Placoid, two Lepidoid, and one Sauroid fish, with well-known muschelkalk species. He also determined the existence of fifteen other species from this deposit, none of which have been yet noticed in the continental Triassic group. Two, if not three, of the above muschelkalk ichthyolites are also found at Aust; and a comparison of the Aust and Axmouth species gives five as common to the two localities, twelve as confined to the former, and two to the latter. The only conclusion, Sir Philip Egerton states, which he feels justified in advancing from the facts adduced in this communication is, that the beds in question, hitherto considered as belonging to the lias, must be removed from that formation, inasmuch as they present a series of fishes not only specifically distinct from those of the lias, but possess in the Ganoid genera the heterocerque tail, an organism confined to the fishes which existed anterior to the lias.

Appended to the paper is a systematic catalogue, compiled from the 'Poissons Fossiles,' of the Ichthyolites hitherto described, from the keuper and muschelkalk of the Continent, together with those recently discovered at the Aust Passage and near Axmouth. The following extract from that document contains the species common to the Continent and England:—

Order.	Genus and Species.	English Localities.	Continental Localities and Formations.	
Placoid.	*Hybodus plicatilis.*	Axmouth.	Passim.	Muschelkalk.
Ganoid.	*Gyrolepis Albertii.*	*Ibid.*—Aust.	Passim.	Ibid.
,,	——— *tenuistriatus.*	*Ibid.*—*Ibid.*	Passim.	Ibid.
,,	*Saurichthys apicalis.*	*Ibid.*	Bayreuth.	Ibid.

A letter, dated Helsingförs, January 5th, 1841, from Professor Nordenskiöld to Mr. Lyell, "On Furrowed Rocks in Finland," was then read.

In consequence of Sefström's observations on the lines which traverse the surface of the Scandinavian mountains *, Professor Nordenskiöld has been induced to attend to similar phænomena in Finland, and he states, that he has noticed lines on almost all mountains from Lapland to the south of Finland, ranging with few deviations from N.N.W. and N. to S.S.E. and S. On the highest cliffs they are seldom visible on account of the surface being worn, but wherever the rocks are overlaid with sand and earth the lines are easily discovered on the covering being removed. Professor Nordenskiöld has likewise discovered shallow furrows, from three to six feet wide, on the surface of the north and south sand-ridges or plateaux which separate the water systems of Finland. He has traced them for more than fifty fathoms maintaining the same directions as the lines upon the mountains, and he has noticed that they are sometimes near each other. The localities mentioned in the letter are—near the church of Kemi; between Antila and Raukula post-stations on the road from Torneå to Uleåberg; and at a place in Carelia, some miles from the iron-works at St. Anna in Suojerfoi parish.

In searching for iron-ore near Helsingförs, a shaft twenty feet deep was sunk in alluvial soil; and Prof. Nordenskiöld observed on the surface of some fragments which had been blasted from a rock at the bottom of the shaft, similar lines to those which occur on the mountains, but he was unable to determine their direction, the excavation being filled with water and mud. As the rock was twenty feet below the surface of the water in the Finnish Gulf, this fact, Professor Nordenskiöld says, proves that the lines which traverse the mountains exist also at least twenty feet beneath the level of the Gulf. Another phænomenon of this nature lately observed by him is a furrowed rock of gneiss, not far from Porkala, but six wersts from the shore. The rock is flat and not very large, and at the height of nine feet above the medium level of the water, is one of the round holes called by the Swedes, "Giants' Pots," but of an unusually large size, being about three feet by two and a half feet. It was somewhat larger within, and sixteen feet deep. Professor Nordenskiöld had the water and detritus which it contained taken out, and found at the bottom numerous perfectly rounded stones mixed up with the mud. The sides of the Pot were exactly ground, and as resplendent as gneiss can be made. At the east side the brim was somewhat rounded, and well marked with a number of large, flat, east and west furrows, showing, in Professor Nordenskiöld's opinion, "that the stones and waves had on that side driven in the brim at the time of its formation." On the opposite side the margin was quite sharp, as if the rock had been broken away since

* [A translation of which appeared in Taylor's Scientific Memoirs, Part 9.—Edit.]

the cavity had been ground. On the surface of the rock were north and south lines similar to those on the mountains, and they intersected the east and west furrows mentioned above; Professor Nordenskiöld therefore infers that the lines were made subsequent to the formation of the Giants' Pot.

With respect to the level of the water in the Finnish Gulf, the following changes are shown to have taken place. On the little island of Fussaro, some miles from Hangövdd, and in the open sea, a mark which was made in the year 1754 is now twenty Swedish ($19\frac{1}{2}$ English) inches above the medium height of the water; another which was cut in 1800 is about nine inches; and a third excavated in 1821 is about five Swedish inches. At St. Petersburg and Cronstadt it is believed that no change has taken place since 1645.

A letter, addressed to Dr. Buckland by Mr. Thomas Bailey, "On the Gravel Deposits in the Neighbourhood of Basford," was next read.

The parish of Basford is situated in a valley ranging nearly north and south where it enters the great Trent vale. On the eastern boundary, which is a very elevated district, commences an extensive argillaceous bed containing comparatively few pebbles; on the west are the coal-fields of Radford and Bilborough; on the north-west occurs the magnesian limestone which extends beyond Mansfield; and on the north is the elevated tract or ancient forest of Hurwood, occupied by great accumulations of gravel and sand, agreeing in character with those in the neighbourhood of Basford. In the midst of the valley in which Basford is situated are lower ridges of hills, mostly ranging in the direction of the valley, and containing beds of gravel quite as thick (two to eight feet), and interspersed with boulders as large as those found in the hollows or lower parts. Mr. Bailey is of opinion that none of these deposits were accumulated by fluviatile action, or by any uniform agent operating during long periods, but by a tumultuous commotion, when the surface of the earth was in a different state to that which now prevails with respect to hill and dale—the deposits being very unequal in thickness, contorted in position, and composed of materials very irregularly associated as regards nature and size. The transport of the drift in one direction, the author says, appears to have been sometimes checked by a rush from an opposite point, by which means the materials were forced into ridges having an axis of loose sand. Some of these ridges, he conceives, may have been produced by intermediate hollows having been scooped out, and subsequently filled with gravel.

Mr. Bailey does not offer any positive opinion respecting the direction by which the detritus arrived at its present situation, but he thinks it could not have been transported from the S. or S.E., as it contains no pebbles of Charnwood and Mount Sorrel sienite, or of lias or lias-fossils, or of gypsum; nor from any point between S.W. and N.W., on account of the absence of mountain-limestone pebbles, and as the fragments of chert which it contains differ from the chert of the Derbyshire strata. Had the drift come from the west, he

states, it ought to contain detritus from the coal-fields which occupy the whole district between Basford and the Derbyshire limestone hills, whereas he has found only one or two small pieces of what might be called jet. He has also never obtained any specimens of magnesian limestone, though that formation occupies almost the whole country to the north-west and north. Mr. Bailey therefore suggests, that the gravel was drifted from a district between the north and east points.

The mass of these deposits consists of fragments of coarse quartzose rock, frequently tinged in a great variety of ways. Many of the pebbles of sandstone are traversed by white veins which project above the general surface: other specimens are rolled portions of quartzose conglomerates, and the greater part of the materials composing them appear to have been much worn, before they were inclosed in the cement; but some of the fragments have sustained very little abrasion by removal from their native bed, preserving all the sharpness of recent fractures. Small masses of iron-ore are not of unfrequent occurrence. Mr. Bailey has seen only one specimen of mica-slate. Many fragments of trap are found in these deposits and some of them are of considerable size, constituting the largest blocks in the deposit, and sometimes weighing two or three hundred pounds; they are often much worn as well as decomposed on the surface. Fragments of porphyry likewise are not rarely met with; and masses of greenstone, or a compound of hornblende and felspar, are also mentioned by the author.

No freshwater or marine shells have been discovered in these accumulations.

Organic remains derived from other deposits are very rare, with the exception of casts of vegetable origin, Mr. Bailey having found only two impressions of shells. Siliceous fragments of stems of Sigillaria and Stigmaria occur in every pit, retaining more or less indubitable marks of their origin, and occasionally exhibiting on the surface a smooth, coffee-brown-coloured coating. One specimen in the author's possession, measuring nearly four and a half feet in circumference, and weighing about 200 pounds, is stated to retain what appears to be a portion of the original bark.

A large series of specimens accompanied the memoir, and was presented by the author.

A letter, dated February 1840, from Mr. Thompson of Yarrells, near Poole, in Dorsetshire, and addressed to Dr. Buckland, was afterwards read.

The object of this communication is to give an account of a boring in search of water at the Union Workhouse, Longfleet, near Poole.

The first land-spring was tapped at the depth of 36 feet, the surface of the ground being about 90 feet above low tide in Poole Harbour. The water was abundant and rose four feet. The next spring occurred at the depth of 127 feet, and others burst forth at 131 feet, 140 feet, 142 feet, 150 feet, 156 feet, 165 feet, 167 feet and 185 feet, from the surface. They all flowed to the same height, and appeared, Mr. Thompson states, to have been fed from the same

source. The next spring was encountered at the depth of 235 feet, in a bed of white sand; it flowed six feet higher, and was more abundant than any of the preceding. The spring which issued from the bottom bed was still stronger, and rose 2½ feet higher than any of the others, or to within 24 feet of the surface. The lowest pipe introduced into the boring had a diameter of three inches; it was worked with "two-lift pumps," the section-pipe of each being two inches bore, for nine hours incessantly, during which 25,728 gallons were discharged. An analysis of the water was made by Mr. J. H. Cooper, and a gallon was found to contain 9½ grains of solid matter, or 4½ grains of oxide of iron, the remainder consisting principally of common salt with a small admixture of sulphate and carbonate of lime, and a trace of oxide of manganese.

At Hamworthy, near Poole, another boring has been made, within 100 yards of the harbour, to the depth of 314 feet, through a series of beds similar to those penetrated at Longfleet, and a similar series of springs was tapped; but as the boring was commenced at 80 feet lower level, or only 10 feet above low tide in Poole Harbour, all the springs flowed over the surface, though not strongly. This boring was abandoned on account of the sand which accumulated in the pipe.

The following is a section of the strata at the Longfleet Union Workhouse:—

No. of the Bed.	Nature of the Bed.	Thickness. Feet. In.	Depth from the Surface. Feet. In.
1.	Black dirt and sand	4 0	4 0
2.	Gravel	4 0	8 0
3.	Fine yellow sand	15 0	23 0
4.	Clay	2 3	25 3
5.	Fine yellow sand	3 6	28 9
6.	Fine brown sand	2 3	31 0
7.	Coarse brown sand and water	5 6*	36 6
8.	Clay	1 6	38 0
9.	Hard fine blue sand	19 0	57 0
10.	Ditto, with white pebbles	2 9	59 9
11.	Hard coarse blue sand	1 3	61 0
12.	Fine blue sand	22 0	83 0
13.	Coarse dark red sand	1 0	84 0
14.	Sulphuret of iron	0 3	84 3
15.	Dark brown clay	1 6	85 9
16.	White clay and sand	3 9	89 6
17.	Light blue clay	21 0	110 6
18.	Dark blue clay	2 0	112 6
19.	Light blue clay	4 0	116 6
20.	Light blue rock	2 0	118 6
21.	Red sandstone	1 6	120 0
22.	Light blue rock	2 0	122 0

* The strata marked with an asterisk are those which threw out springs.

No. of the Bed.	Nature of the Bed.	Thickness. Feet. In.	Depth from the Surface. Feet. In.
23.	Light blue clay	1 0	123 0
24.	Coarse light blue sand*	4 0*	127 0
25.	Dark blue clay	1 0	128 0
26.	Coarse light brown sand	3 6*	131 6
27.	Dark blue clay	3 0	134 6
28.	Black sand and petrified wood	2 0	136 6
29.	Light brown clay	3 0	139 6
30.	Fine blue sand	0 6*	140 0
31.	White clay and yellow sand	2 0*	142 0
32.	Rock (metallic)	0 8	142 8
33.	Light brown sand	7 6*	150 2
34.	Very hard light rock	1 0	151 2
35.	Fine blue sand	5 0*	156 2
36.	Fine blue sand and clay (with pebbles)	5 0	161 2
37.	Light brown sand-rock	4 0*	165 0†
38.	Dark brown sand	2 0*	167 0
39.	Yellow sand and dark brown clay	4 0	171 0
40.	Yellow sand and white clay	0 6	171 6
41.	Dark brown clay	6 6	178 0
42.	Dark clay and black sand	7 0*	185 0
43.	Light brown clay (hard crust every 6 in.)	44 0	229 0
44.	Hard blue *sand-rock* (blue clay in laminæ)	2 0	231 0
45.	White sand	4 3*	235 3
46.	Pipe-clay	15 0	250 3
47.	Black clay	6 9	257 0
48.	Brown clay	3 0	260 0
49.	Stone	1 0	261 0
50.	White sand	5 0	266 0
	Total	266 2	

A letter, dated Glasgow, January 16, 1841, from Mr. Craig to Dr. Buckland, "On the Boulder Deposits near Glasgow," was also read.

The sand- and gravel-beds of the banks of the Clyde are found, Mr. Craig states, in many places besides the adjacent districts; and though wherever he has examined them they are superimposed on the till, yet he does not know if they always occupy that position. At Chapel Hall, at the height of 350 feet, a bone of a Mammoth or Mastodon was found in a bed of laminated sand containing quartz pebbles with fragments of coal-measures and overlying till. Similar beds of sand occur near Eagleham, twelve miles south of Glasgow; and near Galston in Ayrshire, at the height of 500 feet. The sand-beds near Toll Cross on the Hamilton road extend nearly to Broom House Toll, where they rest on till. East of Glasgow the sands lie in the form of a dyke between beds of clay, and extend from the river to the College, where they are cut off by the whinstone dyke

† The inches are omitted in the MS.

which ranges through the city. On the other side of the Clyde they reach as far as Mr. Dixon's iron-works, but further down the river their thickness is not great. At Mr. Smith's property of Whiteinch twelve feet of sand overlie thirty of soft clay and sand.

The bed containing recent shells at the entrance of the Arkleston Tunnel*, near Paisley, is 80 feet above high-water level; and a similar bed at Port Glasgow is 40 feet. In both instances it is overlaid by laminated sands similar to those at Toll Cross, and on which the greater part of the city is built. Their highest level in Glasgow is about the same as that at Arkleston.

The boulders are found almost throughout the basin of the Clyde where denudation has not taken place subsequent to their deposition, except in the elevated trap districts, where they are very rare; the only instances mentioned in the paper are the Baker's Reservoir on the summit of the trap range of Kilpatrick, and Cochney Loch; nor do they appear on those strata which have been upheaved by trap. On the side of the road near Cartlane Bridge is an accumulation of very large primary and transition boulders without till, forming a kind of escarpment on the brow of a very precipitous bank leading to the valley of the Tee. The opposite slope is crowned with beds of sand. The level of these beds is stated to be 550 feet above the sea.

Near the source of the Avon is a deposit of sand and gravel 50 feet thick; and similar beds occur at Greenock Mains, on the Ayr road from Muirk Kirk. West of these deposits, on White Haugh Water, are enormous beds of till. Boulders or fragments of compact flesh-coloured felspar and reddish porphyries are very rare in this district, but are common in the higher parts of Lanark and Ayr, where these rocks form dykes or beds in old red sandstone. Mr. Craig has never found the slightest trace of the coal-measures north of the trap which forms its northern boundary,

The following is a summary of the author's observations respecting the nature of the boulders at Bell's Park, and the extent to which they have been scratched:—

Greywacke, similar to that which is associated with slate near Rose Neath, and mica-schist above Lass on the borders of Loch Lomond. Blocks very abundant, generally smooth, angles rounded, scratches longitudinal, seldom or never crossing each other.

Porphyritic traps and basalts are next in abundance; a few of the basalt-blocks are scratched, but none of the porphyritic.

Granite.—The felspar large-grained; masses few in number, much rounded, very smooth, not scratched.

Old red sandstone and conglomerate.—Abundant, much rounded, never scratched. The conglomerate blocks are very like a variety near Glen Sannox in Arran.

Quartzose rock.—Blocks not abundant, very smooth; more rounded than any other, not scratched.

Coal sandstone.—Blocks angular, scratched longitudinally. Fragments of iron-stone rare and angular, but smooth.

* At this tunnel a bed of coal is stated to have been changed by trap into a bed of pyrites; but a stratum of limestone, though only two feet from the trap, is reported to retain its organic remains.

Carboniferous limestone.—The masses belonging to this formation are next in abundance to those derived from the traps. When large they are much scratched longitudinally and transversely; and the angles are sharp.

"A note by Mr. Murchison on a Section and a List of Fossils from the State of New York," by James Hall, Esq., was likewise read.

Mr. Murchison says, that in consequence of the researches of Mr. Featherstonaugh, Mr. Conrad, Mr. Hall, Mr. Vanuxem, Mr. R. C. Taylor, and other geologists, large tracts in the British colonies in North America and in the United States have been for some time known to be composed of formations containing Silurian, old red sandstone and carboniferous fossils. Mr. Hall's section, presenting a tabular view of the succession of formations, commences with the red sandstone of Blossburgh in Pennsylvania, proved to be the representative of the old red sandstone or Devonian system of Great Britain, in consequence of its inclosing remains of *Holoptychius* and *Coccosteus*. This deposit is succeeded in descending order by others, referable, on account of their testaceous remains, to the lower part of the same system, and these are again underlaid by limestones and shales, especially at Lockport and Rochester, charged with *Ptilodictya lanceolata* and other Silurian corals and fossils. The lowest deposit alluded to by Mr. Hall is the Medina sandstone. The following sectional list, in descending order, is copied from his communication:—

Red sandstone.
Sandstone and shale, abundance of fossil shells.
Shale, with thin layers of sandstone; Fucoides, abundance; few shells.
Green and black shale, several hundred feet thick.
Black shale.
Moscow shale.
Encrinal limestone.
Ludlowville shale.
Thin mass, with Bellerophons.
Shale.
Thin limestone, with fossils.
Marcellus shale.
Limestone, with hornstone.
Onondaga limestone.
Onondaga saliferous group, containing gypsum and salt-springs.
Lockport limestone.
Rochester shale.
Limestone.
Green shale, with fossils.
Pentamerus limestone.
Green shale and iron ore.
Red and grey sandstone, Medina sandstone*.

With respect to the Onondaga saliferous group, Mr. Murchison

* For detailed accounts of the New York Devonian and Silurian Systems and their Organic Remains, see the Geological Reports of the State for 1838, 1839, 1840.

points out its extremely low geological position, resting upon a calcareous stratum, which has been proved by its organic remains to be the equivalent of the Wenlock limestone; and he states that it is of higher antiquity than the oldest salt-bearing beds of Russia. Mr. Murchison also alludes to the great value of Mr. Hall's communication in proving the wide application of the palæozoic succession established in Great Britain.

April 21.—A paper "On the Geological Phænomena in the Vicinity of Cape Town, Southern Africa," by the Rev. W. B. Clarke, F.G.S., was read.

Mr. Clarke commences by stating, that having derived no advantage from the labours of previous geologists, his remarks must be regarded as independent of any prior description. He arranges his details under the heads of Physical aspect, Mineral structure, and Geological changes.

1. *Physical aspect.*—The leading physical features are the magnificent serrated mountains called Blue Berg or Hottentot's Holland, which stretch northward for many miles into the interior; and the promontory which extends from Table Mountain to the Cape of Good Hope. Each of these ranges consists of flat-topped masses interspersed with pyramidal or pointed peaks, and separated by deep ravines; and Mr. Clarke states that their outline is evidently owing to deep vertical fissures intersecting horizontal strata, proving also that the Table Mountain is not a solitary example of the feature to which it owes its name. A level area extends from the base of the Blue Berg to the shore; and between the southern termination of that range and Table Mountain, is the low sandy district called the Table Flats, forming an isthmus between Table Bay and False Bay. A prominent but subordinate physical feature is the Lion's Hill, situated below Table Mountain; at the entrance of the Bay is Robben's Island, and between the base of the Blue Berg and the shore is a low range of hills of limited extent.

False Bay is bounded on the west by the Cape Promontory, and on the east by a continuation of the Blue Berg, presenting the same physical structure and geological aspect as the Promontory.

Mineral structure and position.—In detailing the composition of the rocks and the associated phænomena, Mr. Clarke describes separately, and in the following order, first, each of the principal physical masses; namely, the Lion's Rump, the district between Green Point and Camp's Bay, that between Camp's Bay and Cobler's Hole, the Kloof, and Table Mountain; and secondly, the modern deposits, springs and detritus.

The Lion's Rump.—The lowest deposit at the Lion's Rump varies in characters from a glossy soft slate to a hard and siliceous, as well as a crystalline schist, and occasionally to a rock as close-grained as bassanite. The partings of the masses are stated to be frequently lined with a substance resembling soapstone and serpentine; and the intersection of the joints giving the strata a somewhat columnar structure, the rock presents a basaltic aspect. The true line of bedding is not easily to be distinguished, but the author is of opinion

that it ranges between north and south. The cleavage, where it can be detected, is nearly perpendicular to the horizon. Overlying these schists and composing the upper part of the hill, is a yellowish, argillaceous and sandy laminated rock, which presents the same jointed structure and cleavage as the schist; and owing to the intersection of the joints, the beds divide into wedge-shaped masses, or regular prisms with pyramidal terminations. Mr. Clarke is of opinion that the subjacent schistose rocks have been intruded into or amongst these upper beds, and he states that the grooved and fluted surfaces betray the intensity of the forces with which the slaty masses were ground against each other. He mentions a quarry below the Lion's Rump at the back of Cape Town, as an example of the disturbed position of the schist and overlying sandy rock. He suggests that the schists may belong to the Cambrian, and the superjacent beds to the lower portion of the Silurian system.

The schistose rocks occur also in Robben's Island; and on the other side of the Lion's Rump they form a reef of hard rock along the shore, occurring at intervals at the bottom of Table Bay, and re-appearing in the rounded low range upon the opposite coast. Grooves and scratches, as well as ripple-marks, are very prominent on many of the slabs.

2. *District from Green Point to Camp's Bay.*—The rocks which form the general base of the Lion's Hill are stated to be best examined along the flat shore which skirts it, and where the successive formations crop out. The slate rocks gradually attain a nearly vertical dip as they recede from the Lion's Rump; and between their outcrop in the sea, where they form the first line of barrier rocks, and Green Point, they first change into mica-slate, which soon becomes charged with hornblende, then presents a mottled aspect, and gneiss is ultimately exposed in contact with granite. At the immediate junction of the gneiss with the granite the former is stated to be in some places superficially black and vitreous, extremely hard, as vesicular as lava, and to be most curiously contorted. Masses of true Lydian stone and other metamorphic rocks are stated to be intercalated between the ridges of slate. The true beds in the vicinity of the gneiss range from S.E. to N.W., but where that rock first appears, the strata, as well as the sandstone, dip under the Lion's Rump at an angle of 82° towards the N.E. One line of joints, called by the author cleavage-joints, is stated to be inclined 18° to the W.S.W.; and some of the intercalated beds are said to have similar joints dipping 23° to the N.W. Directly under the Lion's Head, where the gneiss is in contact with the granite, the beds alter in their direction about 5° to the west, the cleavage joints changing also to a range of 30° to the west; and the strata on the shore are in utter confusion. At this point commences a series of highly curious quartz veins, which intersect the gneiss, passing in some places through the joints, as if of posterior origin to the change which produced that structure in the rock, and they throw off from each side numerous branch veins, often at right angles to the main vein. The gneiss is described as overlaid by granite, and the quartz veins to be

most numerous adjacent to it. Veins of granite are likewise visible on the shore, intersecting the gneiss near the junction of the two formations; and numerous instances of the entanglement of the granite and gneiss were noticed by the author, the fragments of the latter, inclosed in the former, being almost invariably coated by quartz. It is also stated, that veins of quartz traverse the entangled portions exactly in the same manner as the solid mass of gneiss; and that the entanglement is nearly always in connexion with the joints, pointing out, Mr. Clarke is of opinion, that all these phænomena are due to one cause; and he is further of opinion, that the silica in the quartz veins was deposited from a state of vapour. The granite is generally large-grained; but where it forms veins, either in the gneiss or in the great mass of granite, it becomes finely grained.

The whole of the shore from this point is granitic, as well as the Lion's Hill, except the cap of sandstone. The junction of the granite and the gneiss cannot be satisfactorily traced owing to the covering of vegetation, but the granite is flanked by nearly vertical or upturned beds of gneiss and slate, and is capped by horizontal beds of sandstone, which are penetrated by granite veins. These phænomena, Mr. Clarke states, clearly establish the induction, that though the periods may have been distant, the schistose rocks owe their elevation to the up-burst of the granite before the deposition of the sandstone; and that subsequently the granite has been re-heated and further elevated, carrying with it the whole area described to a higher level.

3. *Camp's Bay and Cobler's Hole.*—Granite extends along the shore and around Camp's Bay, lining the edge of the sea with huge rounded blocks, and the masses *in situ* are traversed by deep fissures. Near the cottages situated on the road which winds round the middle portion of the Lion's Head and passes over the Kloof to Cape Town, two trap dykes intersect a soft decomposed granite. Under the Lion's Head at Cobler's Hole, and 400 feet above the level of the sea, the granite ledges, for a vertical height of 10 or 12 feet and 30 yards in horizontal extent, are stated to be covered with shingly soil or an elevated beach, having imbedded shells of the same species as now inhabit the neighbouring ocean; and they are so firmly fixed in the soil or to the granite pebbles, as to require some trouble to extract them entire.

4. *The Kloof.*—A vein of trap near the summit of the pass traverses the granite, which is there also in a state of decomposition. About six feet above the road, the dyke is interrupted by a horizontal shift of eighteen inches to the west. At the Kloof is another dyke, which is described in Dr. Abel's work*.

5. *Table Mountain.*—The eastern side of this mountain is formed of granite for nearly two-thirds of its vertical height. On the flat ground at Wynberg occur large blocks of granite perfectly rounded, and the granite floor has the same smooth and rounded surface. The flat between Wynberg and Constantia has also a substratum of granite, with a covering of blown sand or vegetable soil.

* Narrative of a Journey in the Interior of China, &c., 1818.

On the opposite side of False Bay and the Cape Flats granite again rises into lofty mural precipices, capped, as on the west side, by sandstone. It therefore constitutes the fundamental rock of the whole of the district south of the Lion's Head, and is everywhere, except at the flats between Table and False Bays, crested by horizontal beds of sandstone. The soil of the vineyards of Cape Town and Constantia is derived from the decomposition of the granite; and the clay of which it consists is either overlaid or contains a hard layer consisting of quartz pebbles and ferruginous matter. Mr. Clarke does not class this clay, occasionally 100 feet thick, with modern or recent formations, because it occurs at the Lion's Head in gullies, whither it could have been transported only by causes no longer in operation; it is moreover everywhere covered with enormous blocks of sandstone, and occasionally of granite, but is not mingled with them, except at the Kloof, and in the beds of the excavating torrents; he is therefore of opinion that it was accumulated during the period when the whole mass of granite lay beneath the waters of the sea.

The sandstone which enters so largely into the geological constitution of the Cape district, and forms the upper part of Table Mountain, has not yet yielded any organic remains; but in a very similar sandstone, resting upon granite, at Cedar Berg and other parts of the colony, true Silurian Trilobites, with other fossils of that age, have been found*. The Cape sandstone is stated to resemble in mineral character the Caradoc more than the old or new red sandstone, and the altered portions are said to be closely analogous in appearance to the Lickey quartz rock. Many of the beds are soft and white; others are hard, dark-coloured, and very ferruginous; and some are composed of a quartzose conglomerate. Large hollows or excavations, such as exist where the sea beats against a sandstone cliff, appear in all the sandstone escarpments, plainly showing, Mr. Clarke observes, that the sandstone of Table Mountain was once a cliff acted upon by the sea, and the boulders of that rock on the slope beneath bear, he says, unequivocal signs of the action of currents of water. No beds decidedly calcareous were observed by Mr. Clarke; but he mentions a stalactite forty feet long exhibited at Constantia, and the occurrence of similar stalactites on the sides of the mountain at Houts Bay, as indications of the existence of bands of limestone or calcareous sandstone. He also alludes to the two hills of limestone at Cape Agulhas.

Modern Deposits.—These are confined to the dunes along the coast at the foot of the Blue Berg, the sand ridges on the Cape Flats, and the drift sand on the wide space under the slope of the Cape Promontory towards Constantia. Mr. Clarke also includes in them the concretions or calcareous sand tubes formed around the roots and stems of marine plants near Green Point, and at other localities. These accumulations generally assume the form of an elongated tumulus, and are occasionally from 30 to 100 feet thick.

* For an account of Dr. Smith's discoveries at Cedar Berg see Mr. Murchison's Silurian System, p. 583. 1839.

The author also alludes to the vegetable and other debris brought together by the rains, and to the commencement by this means of an embryo lignite formation on one side of the Cape Flats.

Springs.—The well-water in Cape Town is considered unwholesome. Under Table Mountain is a spring which rises from the granite, and is computed to throw out daily 150,000 gallons; and at Newlands near Wynberg is a spring of sufficient volume to work two mills, and to discharge daily 850,000 gallons. That these springs are not the result of accumulations from the heights, is proved, Mr. Clarke says, from their not varying with the season, and because the water cannot be made to rise above the level at which it appears.

Detritus.—The accumulations described under this head are entirely local, being derived from the subjacent or neighbouring rocks. The smooth and rounded granite boulders also do not extend beyond the range of the granite, but Mr. Clarke is of opinion that the ancient currents which flowed over the Cape Flats may have assisted in their partial removal, and may have rounded some of them. In the interior, masses of granite, similar to the Tors of Dartmoor, are stated to occur.

Geological changes.—The first points noticed by Mr. Clarke, are the protrusion of the granite through the slates at the Lion's Head, the consequent vertical position of the schistose beds, the occurrence of fragments of granite in blocks of sandstone; and the proofs deducible from the granite veins which penetrate and alter the gneiss, as well as traverse the superincumbent sandstone, of the granite, since its first elevation, having been re-heated. He also alludes to the quartz veins which are crossed by others of the same nature, as evidences of there having been two periods of action during which the rock was fissured and veins formed; and to the trap dykes, as proofs of igneous activity since the consolidation of the granite. He likewise mentions the softening or the decomposition of the granite where traversed by trap dykes.

The author next describes the changes in the relative level of land and sea. Everything, he says, tends to confirm the inference, that the whole country was at a comparatively recent period under water. Thus the shingle beds, resting upon granite, at Cobler's Hole, prove an elevation of at least 400 feet since the present species of testacea inhabited the adjacent seas; and he adds, "The water-worn masses of sandstone and the hollows in the beds of that rock *in situ*, identical with those now produced by sea-waves beating against a cliff, equally prove the condition of previous elevation; and the steep sides of the granite, in parallel lines of coast, also lead to the conclusion that they were so modified by currents acting in lines coincident with their direction." The occurrence of marine shells in the sand at the Cape Flats likewise shows that the sea once covered that district; and the grooves and scratches at the Lion's Rump, Mr. Clarke observes, justly lead to the inference of elevation. Before the commencement of these changes in the relative level of land and sea, False Bay and Table Bay must have been united by a sheet of water more than

sixty fathoms deep, extending over the flats, and the Cape Promontory must have been an island. To the action of the sea at that period Mr. Clarke attributes the production of the felspathic clay, and its accumulation at the Lion's Rump; and to the action of currents at an earlier period, when the summit of the Table range lay as islands and reefs not far above the level of the sea, the removal of the sandstone and the excavation of the granite at the Kloof, also the denudation and rounding of the ridge of the Lion's Hill, the denudation of Robben's Island, and the production of those terraces, which from the summit of Table Mountain appear to stretch gradually downwards to the Cape of Good Hope. The separation of the Lion's Rump and the Devil's Mountain from the Table Mountain, and the fissures throughout the range, the author conceives were produced during the elevation of the country. Proofs of changes of relative level of sea and land are stated to be equally apparent in the interior; and Mr. Clarke says, that the inspection of an accurate map will convince the inquirer, that Southern Africa must have been an Archipelago. In conclusion, some general observations are made on the great similarity in the geological composition of Southern Africa and New South Wales.

May 5.—A Memoir "On the Distribution of the Erratic Boulders, and on the contemporaneous unstratified Deposits of South America," by Charles Darwin, Esq., F.R.S., F.G.S., was read.

The extensive regions more particularly noticed in this paper are the plains traversed by the Rio Santa Cruz (lat. 50° S.); Tierra del Fuego, including the coasts of the Strait of Magellan, and the Island of Chiloe (lat. 43° S., long. 73° W.).

Patagonia.—Between the Rio Plata and the Rio Santa Cruz, Mr. Darwin did not observe any boulders, and the only one he noticed in ascending the first 100 miles of the latter river was a mass 7 feet in circumference, about 57 miles from its mouth, or 100 from the Cordillera. At 100 miles from the coast, or 67 from the nearest slope of the Cordillera, transported blocks first occur, and 12 miles nearer the chain they are extraordinarily numerous, consisting of clay-slate, felspathic rocks, chlorite schist and basaltic lava. They are generally angular, and some of them are of immense size, one being 60 feet in circumference, and projecting from 5 to 6 feet above the surface of the ground. The vast open plain on which they lie scattered, is here 1400 feet above the level of the sea, and its surface is somewhat irregular, owing partly to denudation and partly to the protrusion of hummocks and fields of lava. The plain slopes gently and regularly towards the Atlantic, where the sea-cliffs are about 800 feet high; but towards the Cordillera it rises more abruptly, attaining near the chain an elevation of 3000 feet. The highest peaks of the Cordillera in this part of its range do not exceed 6400 feet above the level of the sea. The following section, exhibited in the banks of the Santa Cruz in longitude 70° 50′ W., is given by Mr. Darwin to illustrate the nature of the plain on which the boulders rest.

		feet.
1.	Gravel, or well-rounded shingle, coarsely stratified, bearing chiefly on its surface great angular erratic blocks . .	212
2.	Basaltic lava	322
3.	Variously coloured thin strata, the lower ones containing minute pebbles of the same nature as the boulders, with the exception of the lava	588
		1122
	Bed of the Santa Cruz, above the level of the sea	280
		1402

The shingle bed (1.) extends uninterruptedly to the coast, where it is certainly of submarine origin; and from the general similarity of its nature, Mr. Darwin is of opinion, that it was all accumulated under the same circumstances. The contrast in the means of transport between the deposits (3.) and (1.), the former consisting of fine particles and the latter of large pebbles and immense blocks of the same rocks with the former, is noticed by Mr. Darwin as an interesting circumstance.

The valley of the Santa Cruz widens, on approaching the Cordillera, into an estuary-like plain, which has an elevation of only 440 feet; and it is believed by Mr. Darwin to have been submerged within the post-pleiocene period, because existing sea-shells were found near the mouth of the plain, and because terraces, which, near the coast, certainly are of recent submarine origin, extend far up the valley. Around this estuary-like plain, and between it and the great high plain, is a second plain, 800 feet in height, the surface of which, as well as the bed of the river in this part, consists of shingle with great boulders. Some of these are of granite, sienite and conglomerate, rocks, which were not observed by Mr. Darwin on the high plain; and on the contrary, the boulders of basaltic lava which were so numerous there, were entirely absent from this lower plain and the river-course. From these circumstances, and likewise from the immense quantity of solid matter which must have been removed in excavating the valley of the Santa Cruz, the author infers that the boulders on the intermediate plain and in the bed of the river, between 30 and 40 miles from the Cordillera, are not derived from the wreck of the high plain, but were transported from the Cordillera subsequently to the modelling of the country, and within, or not long before, the period of existing shells.

Mr. Darwin did not observe erratic blocks in any other part of Patagonia, but he states, on the authority of Capt. King, that large fragments of primary rocks occur on the surface of the great plain which terminates at Cape Gregory, in the Strait of Magellan.

Tierra del Fuego, and Strait of Magellan.—The eastern portion of Tierra del Fuego is formed of large outliers of the Patagonian formation, fringed by deposits of more recent origin. These lower plains, varying in height from 100 to 250 feet, have been elevated within the post-pleiocene period; and they consist of finely grained

argillaceous sandstone arranged in thin horizontal or inclined laminæ, and often associated with curved layers of gravel. On the eastern borders of the Straits of Magellan, and at Elizabeth Island, Cape Negro, Nuestra Señora de Gracia, all within the Straits, as well as along the line of coast extending to Port Famine, the sandstone passes into, or alternates with, great unstratified deposits, either of an earthy nature and whitish colour, or of a hardened coarse-grained mud of a dark colour, both containing angular and rounded fragments as well as great boulders of sienite, greenstone, felspathic rocks, clay-slate, hornblende-slate, and quartz. These are arranged without the slightest indication of order, and are derived from mountains at least 60 miles distant to the west or south-west. Sometimes the mass is divided by beds of stratified shingle. North of Cape Virgins, near the entrance of the Strait, it alternates with beds of argillaceous, horizontally laminated sandstone, often thinning out and becoming curvilinear at each end. The inclosed fragments must, in this case, have been transported at least 120 miles. Though Mr. Darwin observed only two boulders imbedded in this deposit, yet as he did not notice any scattered on the surface of the country, he concludes that the boulders which occur in vast numbers on all the beaches have generally been washed out of the cliffs: in St. Sebastian's Bay, however, on the east coast of Tierra del Fuego, he found many blocks in a protected position at the base of a naked cliff 200 feet high, entirely composed of thin strata of finely grained sandstone; he therefore infers that, in this instance, they must have been derived from a thin superficial deposit. From the form of the land where these boulders occur, it is clear, Mr. Darwin states, that long anterior to the present total amount of elevation, a wide channel must have connected the middle of the Strait of Magellan with the Atlantic; and from the occurrence of boulders on the low neck of land near Elizabeth Island, that at the same period a straight channel must have existed between Otway Water and the eastern arm of the Strait. As the present currents off Cape Horn set from the west, Mr. Darwin says, it is probable that the ancient currents had a similar direction, and this inference, he adds, is in accordance with the fact, that the boulders and smaller fragments have been transported from mountains to the west. Navarin Island, and several adjacent islets off the extreme southern parts of Tierra del Fuego, are fringed at about an equal height by an unstratified boulder deposit, very similar to that of the Strait of Magellan; and in Beagle Channel, which separates Navarin Island from Tierra del Fuego, it occasionally alternates regularly with layers of shingle. This extensive deposit resembles, Mr. Darwin states, the "Till" of Scotland, and the boulder formation of Northern Europe and the East of England. The interstratification of regular beds, the occasional appearance of stratification in the mass itself, the juxtaposition of rounded and angular fragments of various sizes and kinds of rock derived from distant mountains, and the frequent capping of gravel, indicate some peculiar but similar origin in this deposit of the above widely separated regions. Mr. Darwin follows Mr. Lyell in belie-

ving that floating ice, charged with foreign matter, has been the chief agent in its formation; but he adds that it is difficult to understand how the finest sediment was arranged in horizontal laminæ, and coarse shingle in beds, while stratification is totally, and often suddenly, wanting in the closely neighbouring till, if it be supposed that the materials were merely dropped from melting drift ice; and he is disposed to think that the absence of stratification, as well as the curious contortions described in some of the stratified masses, are mainly due to the disturbing action of icebergs when grounded. He believes also that the total absence of organic remains in these deposits may be accounted for by the ploughing up of the bottom by stranded icebergs, and the impossibility of any animal existing on a soft bed of mud or stones under such circumstances. In confirmation of the disturbing action of icebergs, Mr. Darwin refers to Wrangell's remarks on their effects off the coast of Siberia.

Chiloe.—North of latitude 47° and between it and the southern extremity of Chiloe, the author landed at several points, but saw no boulders; and he explains their absence by the coast being at a distance from the Cordillera, and separated from it by intervening high land. At Chiloe erratic boulders, often of great size and consisting of granite and sienite, occur in vast numbers along the whole line of the eastern and northern beaches, as well as on the islets parallel to the eastern coast, and on the land at the height of upwards of 200 feet; but the author did not observe any on the western coast at the two points which he examined, nor during an excursion of 30 miles across the high central portion of the island. Chiloe consists, as far as Mr. Darwin ascertained, of mica-slate and volcanic formations, extensively bordered, but chiefly on the eastern and northern sides, by a horizontally-bedded tertiary sandstone and volcanic grit. On the eastern coast, the land is indistinctly modelled into successively rising plains, the surfaces of the upper and the whole thickness of some of the lower being in general composed of stratified shingle. A few boulders occur in this gravel; and as the shores have been extensively denudated, Mr. Darwin infers that most of the very numerous blocks on the beaches were originally included in it. At the northern end of the island, the granitic and sienitic boulders are intermingled, but 30 miles to the southward, the author noticed only granite blocks. The parent rock he believes lies in the Cordillera; and several of the varieties of granite and sienite at the northern end of the island are stated, on the authority of an intelligent resident, to form whole mountains in Reloncavi Sound, on the opposite part of the main land. The larger masses were quite angular, and resembled fragments at the foot of a mountain. One block measured 15 feet in length, 11 in breadth, and 9 in height; another, of a pentagonal form, 11 feet on each side, and at one part projected 16 feet above the sand, in which it was partly buried.

At the extreme northern point of Chiloe, a headland 250 feet high is joined to Lacuy peninsula by a low neck of land; and from its composition, height and stratification, Mr. Darwin ascertained that it was once continuous with the opposite coast. The boulders

were much more numerous on the isthmus and its sides at the height of 150 feet, than on any other part of the surrounding country; and as the sea must have flowed over this isthmus in a channel, previous to the amount of elevation, ascertained to have taken place here within the post-pleiocene period, the position of these boulders proves, according to Mr. Darwin, even more clearly than the cases occurring in Tierra del Fuego, the evident relationship between their distribution and the lines of anciently existing sea-channels. In the southern half of Chiloe, and on one of the Chonos islands, the author discovered a deposit of hardened mud, including far transported, angular and rounded fragments, and resembling the till of the Straits of Magellan. In a layer of loose sand at the base of the cliff in the latter locality, he noticed a quantity of comminuted marine shells with a fresh aspect; and at Chiloe he also observed, at a point where a mass of till passed into finely grained laminæ, small fragments of a Cytheræa.

With respect to the age of the boulder formation of Chiloe, Mr. Darwin offers no precise remark, but he says that it probably occurs within the post-pleiocene period, because at a height of 350 feet on the peninsula of Lacuy, and therefore considerably above the level of this formation, a great bed of existing sea-shells was observed, and neither the boulder nor accompanying beds appear to have been of deep-water origin. Similar evidence was adduced respecting the age of the till of Tierra del Fuego. North of 41° 47′ S. lat., Mr. Darwin did not observe on the Pacific side of South America either boulders or till; nor any north of the Straits of Magellan, on the shores of the Atlantic side; and he accounts for the absence of erratic blocks in the latter region by its great distance from the Cordillera. He is also strongly of opinion that till will be found to be limited to the latitudes in which true boulders occur.

Glaciers, &c.—In the concluding part of his memoir, the author offers a few remarks on the glaciers of Tierra del Fuego, and on the transport of the boulders. He did not disembark on any glacier, but in the Beagle and Magdalen channels he passed within 2 miles of several. The mountains were covered with snow, and the glaciers formed many short arms, terminating at the beach in low perpendicular cliffs of ice. Their surface, to a considerable height on the mountains, was perfectly clean and of a bright azure colour; and the former condition he ascribes to their shortness, to their not being flanked by overhanging precipices, and to their not being formed by the junction of two or more smaller streams. The descent of the glaciers, Mr. Darwin states, cannot be very slow, as vast masses continually break off with a great noise, and produce a tumultuous surf on the adjacent beaches. The glaciers in the Beagle Channel were generally bordered by a tongue of land composed of huge fragments of rock, and many boulders were strewed on the neighbouring shores. The glacier which he approached most closely descended to the head of a creek formed on one side by a wall of mica-slate, and on the other by a broad promontory from 50 to 60 feet high, on which he landed: it appeared to consist en-

tirely of enormous masses of granite. This promontory, he conceives, was once a lateral moraine, and as it projects nearly half a mile beyond the extremity of the glacier, and is covered with old trees, he infers that the glacier has diminished in length to that extent.

Mr. Darwin says it is impossible to explain the distribution of boulders without the agency of ice, but he adds, that neither the till of the Strait of Magellan which passes into, and is irregularly interstratified with, a laminated sandstone containing marine remains, nor the stratified gravel of Chiloe, can have been produced like ordinary moraines. The boulders, likewise, on the lower levels at the head of the Santa Cruz river, he considers, could not have been distributed in their present position by glaciers, the surface having been modelled by the action of the sea; and the little inclination of the high plain from the ridge of the Cordillera to where the boulders occur, as well as the absence of mounds or ridges on it, and the form of the fragments, render it very improbable that they were propelled from the mountains by ancient glaciers. Hence, he concludes, that the blocks of Tierra del Fuego and Chiloe were certainly transported by floating ice, and most probably those of the low and high plains of Santa Cruz. Finally, he is of opinion, from the general angularity of the blocks, and from the present nature of the climate of the southern parts of America, which favours the descent of glaciers to the sea in latitudes extraordinary low, that it is more probable that the boulders were transported on the surface of icebergs, detached from glaciers on the coast, than imbedded in masses of ice, produced by the freezing of the sea.

May 19th.—A paper "On the Agency of Land Snails in corroding and making deep Excavations in compact Limestone Rocks," by the Rev. Professor Buckland, D.D., F.G.S., was first read.

During the meeting of the Geological Society of France at Boulogne, in September 1839, Dr. Buckland's attention was called by Mr. Greenough to a congeries of peculiar hollows on the under surface of a ledge of carboniferous limestone rocks. They resembled at first sight the excavations made by Pholades, but as he found in them a large number of the shells of *Helix aspersa*, he inferred that the cavities had been formed by snails, and that probably many generations had contributed to produce them*.

A few years since, the Rev. N. Stapleton informed the author that he had discovered at Tenby, in the carboniferous limestone on which the ruins of the castle stand, perforations of Pholades 30 or 40 feet above high-water level; but having recently examined the spot, Dr. Buckland ascertained that these excavations were the work of the same species of Helix as that which had formed the cavities in the limestone near Boulogne, and he found within them specimens of the dead shells as well as of the living. The mode of operation by which the excavations were made, he conceives, is the same as that by which the common limpet (*Patella vulgata*) corrodes a socket in

* See Bulletin Geol. Soc. France, vol. x. p. 434, 1839.

calcareous rocks, and he is of opinion that the corrosion is due to the action of some acid secreted from the body of the limpet or helix.

That the perforations, both at Boulogne and Tenby, were not the work of Pholades, Dr. Buckland says, is evident,

1st. From their size and shape, which, instead of the straight and regular form accurately fitting the shell of the animal by which each hole was perforated, are tortuous, irregularly enlarging and contracting, and rarely continuous in a straight line. The holes moreover are often separated by only a thin partition, or are confluent.

2ndly. Because they are wanting on the upper surface of the projecting ledges of limestone, whilst on the sides and lower surfaces of the ledges they are excavated to considerable depths.

The above reasons, Dr. Buckland says, against the excavations having been made by any marine lithophagous animal, are favourable to the hypothesis which refers the production of them to snails. These animals, he observes, could find shelter only on the margin and lower surface of the projecting rock, and the irregular form of the confluent cavities correspond with that of the clusters of snails in their ordinary latitat and hybernation; and if to these reasons be added the fact of finding both living and dead shells in the excavations, the evidence, the author conceives, is decisive as to the agency of snails in producing the phænomena under consideration.

In conclusion, the author offers some remarks on the means by which these hollows have been corroded having been overlooked, in consequence, he suggests, of their having been probably referred to the action of the weather, or water, or to original irregularities in the composition of the stone.

A paper "On Moss Agates and other Siliceous Bodies," by John Scott Bowerbank, Esq., F.G.S., was then read.

In a paper "On the Origin and Structure of Chalk-flints and Greensand Cherts*," Mr. Bowerbank inferred that the sponges from which he conceives those bodies originated, differed from recent keratose sponges only in having possessed numerous siliceous spicula. Since that paper was read, the author, however, has found in true keratose sponges from Australia†, as well as in the sponges of commerce from the Mediterranean and the West Indies‡, siliceous spicula in great abundance. All discrepancies, therefore, between the extinct and modern types of a portion of the animals under consideration, he says, is now removed. In these prefatory remarks, Mr. Bowerbank likewise states that there is at present only one known species of recent sponge (*S. fistularis*) the fibre of which is truly tubular.

The author then proceeds to detail the evidences of the existence in moss agates from Oberstein and other parts of Germany, as well as from Sicily, and in green jaspers from India, of the remains of sponges, in the following order: 1st, the proofs of the fibrous structure; 2nd, of the preservation of gemmules; and 3rd, those of the

* See Geol. Trans., 2nd Series, vol. vi. Part 1. 1841. Proceedings, vol. iii. p. 278, 1840. [and Phil. Mag., Third Series, vol. xviii. p. 220.]

† Annals of Nat. Hist., April 1841.

‡ Microscopic Journal, vol. i. No. 1, p. 8, 1841.

existence of vascular structure. The specimens were examined as opake objects, with direct light concentrated by a convex lens. The number of agates amounted to nearly 200, and that of green jaspers to about 70.

1. *Fibrous structure.*—Though polished agates afforded Mr. Bowerbank, in almost every specimen, strong evidence of spongeous origin, yet the structure and arrangement of the fibres were seldom perfectly preserved throughout, presenting every intermediate state from complete decomposition to the most distinct spongeous tissue. The siliceous matrix of these remains exhibited a clear and frequently crystalline aspect, but the prevailing tint of the enclosed organic matter was bright red, brown, or ochreous yellow; occasionally, however, the fibre was milk-white or bright green. The colouring matter was generally confined within the bounds of the animal tissue, leaving its surface smooth and uninterrupted; sometimes it occurred only in the interior of the tubular fibre, the sides being semipellucid or milk-white; whilst in other cases not only the fibre was completely charged with colouring matter, but the surface was also slightly encrusted with it. In an agate believed to be from Sicily, the greater part consisted of a confused mass composed of innumerable bright red fibres with no perceptible remains of surrounding structure, but near the margin of the specimen the tubuli were as perfectly preserved as in a recent sponge, presenting a semi-pellucid and horny-looking substance enveloping red fibres. In those instances in which the red pigment did not appear to have entered the tube, the structure was best preserved, and Mr. Bowerbank states that such ought to be the case, as the fibres of the *Spongia fistularis*, though hollow throughout, are closed near the natural termination. The tubes in the Sicilian agate anastomosed in the same manner as the fibres of the Mediterranean sponge of commerce, and in the places where they were intersected they frequently exhibited the internal cavity. These characters, the author remarks, prove that the red fibre is the cast of the interior of the tube, and its diameter, he adds, is as nearly as possible the same as that of the hollow of the tube. In a moss agate from Oberstein the walls of the best-preserved tubuli were charged with red pigment, and the internal cavity was filled with pellucid silex, while the portion which had suffered most from decomposition was a confused bright red mass with obscure traces of fibrous structure.

In the green jaspers from India the organic remains were found to be generally better preserved than in the moss agates of Germany and Sicily, and admitted of being recognised as distinct species. The green colouring matter was confined, with very few exceptions, within the boundaries of the sponge-fibre, the surrounding matter consisting of minute pellucid radiating crystals. Some of the specimens examined by Mr. Bowerbank were furnished with minute contorted tubuli, very similar to those which are described in his former paper* as occurring upon the surface of chalk-flints. In other

* Geol. Trans., 2nd Series, vol. vi. Part 1. 1841.

specimens the fibres were not disposed in the same manner as in the sponge of commerce, but in a series of thin plates, resembling very much the macerated woody fibres of the leaves of some endogenous plants. Only one recent species, from Australia, is known to Mr. Bowerbank to exhibit this structure.

No spicula are mentioned by the author in either the agates or jaspers, and but one instance of the occurrence of foraminifera. The whole of the sponges contained in the green jaspers, Mr. Bowerbank refers to that division of the keratose which he has called *Fistularia*.

2. *Gemmules*.—A specimen of Indian green jasper, which had undergone so great decomposition as to prevent the original fibrous structure from being detected, presented innumerable globular vesicles of nearly uniform size. Many of them were simple and transparent, and could be recognised as organic only by the regularity of their size and form, and by having universally dispersed over their outer surface minute irregular black particles; but by far the greater number of them had in their interior a globular opake body, about one-third their own diameter. Associated with these vesicles were numerous small fibrous masses resembling minute keratose sponges, the largest of which were five or six times the diameter of the vesicles; but the smallest were identical in nature with the nucleus, though in a higher state of development. In other specimens from the same mass of jasper, larger vesicles were found more sparingly imbedded amidst the fibrous tissue of the sponge. From these characters and their resemblance to those of the ova of some recent sponges, Mr. Bowerbank has little doubt that the vesicles are the fossilized gemmules of the sponges which gave the form to the siliceous masses in which they are imbedded. An agate supposed to have come from Oberstein, presented characters which, Mr. Bowerbank is of opinion, indicated gemmules in an immature state, or in different stages of development, fixed to the fibre of the sponge; and in another specimen, believed to have been received from the same locality, gemmules in different conditions were sparingly scattered amid the tissue.

If this idea of the development of the gemmules *in situ* be correct, it will account, the author thinks, for the frequent occurrence of small detached patches of minute sponge-fibre in well-developed and large-sized tissue. Several other specimens, considered by Mr. Bowerbank to contain gemmules in different stages of development or decomposition, are described in the paper, particularly an agate from Antigua in the possession of Mr. R. Brown; and one from Oberstein, which contained vast numbers of small, pellucid, yellow globules, bearing a strong resemblance to the minute granules which occur in the gelatinous or fleshy sheath surrounding the fibres of the sponge of commerce, and which are probably incipient germs. In accounting for the preservation of the gemmules in a fossil state, Mr. Bowerbank refers to the covering of the ova of birds, fishes and reptiles; and he says, it is natural to expect that the gemmules of the sponge should be similarly protected, and therefore preserved after the decay of the sponge from which they derived their origin.

3. *Vascular structure*.—In a species of recent Turkey sponge, and

in some others from Australia*, Mr. Bowerbank detected in the horny sheath which invested the solid fibre, minute anastomosing vessels; but he has not observed a similar vascular covering on the external surface of the two specimens of *Spongia fistularis* which he has examined. The co-existence, however, of this sheath with a tubular fibre, he states, he has discovered in specimens of Indian green jasper. On examining with a power of 60 linear a thin polished slice, he found that some well-preserved tubes, of greater size than the rest, had, on their external surface, a coating of a darker colour than the other parts of the fibre, and were evidently analogous to the vascular sheath of the keratose sponges of commerce. On employing a power of 500 linear, the presence of a reticulated vascular structure was exhibited as distinctly as in the recent sponge, particularly where a portion of the originally horny or fleshy part of the sheath had undergone a slight degree of decomposition. This structure Mr. Bowerbank has also detected in two fragments of flint-pebbles.

The characters exhibited by this external coating are not the only evidences of vascular structure which the author found during his examination of the organic remains inclosed in moss agates and Indian green jaspers, for he discovered in the centre of the tube which exhibited the sheath, a dark thread penetrating the cavity for a considerable distance, and when examined with a power of 500 linear, it assumed the appearance of a spiral tubular thread, frequently obscured by irregular patches of a substance which the author conceives may have been glutinous animal matter. In another specimen of green jasper the spiral course of this curious tissue was much less obscure, and when examined with a power of 800 linear its tubular nature was evident. The same tissue also lined the cavity of almost every fibre of the sponge which was stated to exhibit a structure composed of foliaceous plates, like the skeletons of the leaves of some endogenous plants. In an agate, probably from Oberstein, Mr. Bowerbank says, he detected other evidences of tissue of an exceedingly remarkable character. The fibre, which was very large, had been apparently surrounded by a villose coat, and wherever, by polishing, a longitudinal section had been exposed, one or two minute vessels of uniform diameter and simple structure were visible in the centre of the fibre, and ranging in the direction of its axis. At irregular distances within these vessels the author discovered pellucid round globules, the diameter of which varied from the 1000th to the 2380th of an inch, the diameter of the vessels ranging from the 1000th to the 2000th of an inch. In other parts of the interior of the fibre were opake or semi-pellucid spheres, and in different portions of the agate were considerable numbers of larger, opake, round bodies, the whole of which Mr. Bowerbank considers to be gemmules in various states of development; and he thinks it is extremely probable that the vessels containing the globules were true ovarian ducts. In support of this inference Mr. Bowerbank describes another agate, in which there were no appearances of well-defined anastomosing

* Microscopic Journal, vol. i. No. 1, p. 10.

fibres, but which exhibited numerous long and simple thread-like fibres apparently much decomposed, as their substance consisted sometimes of a congeries of minute separate particles, and sometimes of straight or curved lines composed of minute black bodies. In other cases these strings of incipient gemmules were contained within the boundaries of the tubes, and then presented rarely more than a row of single gemmules; but occasionally the diameter of the vessels appeared to have been much enlarged, and the gemmules were indiscriminately dispersed within its cavity. In some instances also they exceeded in diameter the vessel or its remains, as if they had outgrown and burst their natural boundary, or the walls of the latter had contracted. From the close resemblance in the structure and contents of these vessels to those contained in the large sponge-fibre first described, Mr. Bowerbank has little doubt, whatever may have been their original nature, that they are the same kind of tissue, under somewhat different conditions.

In all the agates and jaspers which have been microscopically investigated by the author, the spaces not occupied by remains of spongeous texture were filled with silex or chalcedony arranged in bands which conformed more or less to the outline of the enclosed fossil. Where, however, the matrix consisted of radiating crystals, the decayed animal remains frequently appeared to have been impelled forward, in the same manner as the decomposed cellular portions of fossil wood have often yielded to the crystallizing process of the associated mineral matter.

Egyptian jaspers, Mocha stones, &c.—The author has examined also numerous specimens of polished Egyptian jaspers, which, when viewed as opake objects, by direct light and with a power of 150 linear, were found to consist of finely comminuted light buff or brown irregular granules, cemented by semi-transparent silex, very much resembling the state in which it exists in chalk-flints and greensand cherts, and to the variations in its colouring matter the banded appearance of the jaspers is due. Imbedded, but very unequally in the layers composing the jaspers, Mr. Bowerbank discovered hundreds of beautiful foraminifera closely resembling those found in chalk-flints, and often difficult to distinguish from the species found in the Grignon sand of the calcaire grossier.

The Mocha stones which the author has examined, presented no indications of organic structure, the moss-like delineations and other appearances, resembling beautiful, thin, reticulated tissues, being due to dendritical or metallic infiltrations.

In the larger pebbles of a mass of Herefordshire pudding-stone, Mr. Bowerbank discovered the characteristic spongeous structure of chalk-flints.

In conclusion, the author dwells upon the difficulties attending the study of the bodies which he has examined and described, in consequence of the little attention which has been paid, with few exceptions, to the structure of recent sponges; and he states that the aspect of the latter, when viewed by the unassisted eye, is so different from that which it presents when seen under a high micro-

scopic power, that those who have not been accustomed to study recent sponges with that aid would never recognise a similar structure in the fossils described by him. He also shows that the prevalence of keratose sponges over those belonging to the genus *Halichondria* is what might naturally be expected, as the spicula which form the skeleton of the latter would be less likely to be preserved in their original position than the horny fibres of the former.

Lastly, the author alludes to the great share which sponges have had in the production of the solid strata of the earth's crust.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 327.]

January 8, 1841.—Remarks on the Present State of our Knowledge relative to Shooting-Stars, and on the Determination of Differences of Longitude from Observations of those Meteors. By Mr. Galloway.

After adverting to some of the earlier opinions which have been entertained on the nature of fire-balls, shooting-stars, and other igneous meteors, the author remarks that no very definite theory was formed respecting them till towards the end of the last century; for although the cosmical origin of the more remarkable bolides and fire-balls had been suspected, the shooting-stars were generally regarded as atmospherical phænomena, which were ascribed by some to electricity, and by others to the inflammation of hydrogen gas accumulated in the higher regions of the atmosphere. In 1794, Chladni published his celebrated work, in which he gave a catalogue of all the recorded observations of fire-balls; and, from a comparison of the different descriptions, inferred that these meteors have not their origin in our atmosphere, but are cosmical masses moving through the planetary spaces with velocities equal to those of the planets, which, when they encounter the earth's atmosphere, are inflamed by the resistance and friction, and become luminous, sometimes bursting into pieces, and scattering masses of stone and iron on the ground. This opinion was at first greatly ridiculed; but the repeated and even not unfrequent fall of meteoric stones, and the discovery by Howard that all of them present an almost perfect similarity of constitution, widely different from that of any substance found on the earth, at length forced conviction even on the most sceptical. From the close resemblance between fire-balls and shooting-stars, and, indeed, the impossibility in many cases of distinguishing the one class of meteors from the other, Chladni was led also to ascribe a cosmical origin to the latter phænomena. At this period, however, there were no observations from which precise or certain conclusions could be formed respecting the altitudes, velocities, or paths described by the shooting-stars—the elements by which the question of their existence within or beyond the atmosphere could be solved. In the year 1798, the first series of observations for determining these points was undertaken in Germany by Brandes and Benzenberg. Having selected a base-line of about nine English

miles in length, and stationed themselves at its extremities, they began to observe on nights previously agreed on; and when a meteor was seen, they immediately traced its apparent path on a celestial map, noting carefully the exact times of its appearance and extinction, with any other circumstances likely to assist in identifying it. The meteors observed simultaneously at both stations were in this manner recognised with considerable certainty; and the comparison of their paths on the two maps afforded data for the determination of their parallaxes and altitudes. The results were as follows:—Between the 11th of September and the 4th of November, 1798, only twenty-two corresponding observations were obtained from which the altitudes could be computed. The altitude of the lowest was about six English miles; there were seven under 45 miles; nine between 45 and 90 miles; six above 90 miles; and one had an altitude of about 140 miles. There were only two observations from which the velocity could be deduced: the first gave 25 miles, and the second from 17 to 21 miles in a second. The most remarkable result was, that at least *one* of the meteors moved upwards, or away from the earth. By these observations, the perfect similarity between fire-balls and shooting-stars, in respect of velocity and altitude, was completely established.

Another attempt, on a more extensive scale, to determine the altitudes and velocities of shooting-stars by means of simultaneous observations, was made by Brandes in 1823, assisted by a number of associates resident in Breslaw and the neighbouring towns. The observations were continued from April to October, and during this interval about 1800 shooting-stars were observed at the different places, out of which number ninety-eight were found which had been observed simultaneously at more than one station. The altitudes of four of these were computed to be under 15 English miles; of fifteen between 15 and 30 miles; of twenty-two between 30 and 45 miles; of thirty-five between 45 and 70 miles; of thirteen between 70 and 90 miles; and of eleven above 90 miles. Two of these last had an altitude of about 140 miles; one of 220 miles; one of 280 miles; and there was one whose height was computed to exceed 460 miles. Thirty-six orbits were obtained; in twenty-six of which the motion was downwards, in one horizontal, and in the remaining nine more or less upwards. In three cases only the observations were so complete as to furnish data for determining the velocity; the results were respectively 23, 28, and 37 English miles in a second, the last being nearly double the velocity of the earth in its orbit. The trajectories were frequently not straight lines, but incurvated, sometimes horizontally, and sometimes vertically, and sometimes they were of a serpentine form. The predominating direction of the motion was from north-east to south-west, contrary to the motion of the earth in its orbit,—a circumstance which has been generally remarked, and which is important in respect of the physical theory of the meteors.

A similar set of observations was made in Belgium in 1824, under the direction of M. Quetelet, the results of which are published in

the *Annuaire de Bruxelles* for 1837. M. Quetelet was chiefly solicitous to determine the velocity of the meteors. He obtained six corresponding observations from which this element could be deduced, and the results varied from 10 to 25 English miles in a second. The mean of the six results gave a velocity of nearly 17 miles per second, a little less than that of the earth in its orbit.

The last set of corresponding observations referred to in the paper was made in Switzerland on the 10th of August, 1838; a circumstantial account of which is given by M. Wartmann in Quetelet's *Correspondance Mathématique*, for July 1839. M. Wartmann and five other observers, provided with celestial charts, stationed themselves at the Observatory of Geneva; and the corresponding observations were made by M. Reynier and an assistant at Planchettes, a village about 60 miles to the north-east of that city. In the space of seven and a half hours, the number of meteors observed by the six observers at Geneva was 381; and during five and a half hours the number observed at Planchettes by two observers was 104. All the circumstances of the phænomena—the place of the apparition and disappearance of each meteor, the time it continued visible, its brightness relatively to the fixed stars, whether accompanied with a train, &c., were carefully noted. The trajectories were then projected on a large planisphere. The extent of the trajectories described by the meteors was very different, varying from 8° to 70° of angular space, and the velocities appeared also to differ considerably; but the average velocity concluded by M. Wartmann was 25° per second. It was found, from the comparison of the simultaneous observations, that the average height above the ground was about 550 miles; and hence the relative velocity was computed to be about 240 miles in a second. But as the greater number moved in a direction opposite to that of the earth in its orbit, the relative velocity must be diminished by the earth's velocity (about 19 miles in a second). This still leaves upwards of 220 miles per second for the absolute velocity of the meteor, which is more than eleven times the orbital velocity of the earth, seven and a half times that of the planet Mercury, and probably greater than that of the comets at their perihelia.

From the above results, it is obvious that the heights and velocities of the shooting-stars are exceedingly various and uncertain; but if the observations are in any respect worthy of confidence, they prove that many of these meteors (according to Wartmann's observations, by far the greater number) are, during the time of their visibility, far beyond the limits to which atmosphere is supposed to extend, and that their velocities greatly exceed that which is due to bodies moving at the same distance from the sun under the influence of solar gravitation.

It is perhaps impossible to form any correct estimate of the absolute magnitudes of the meteors. Their apparent magnitudes differ greatly; the greater number resembling stars of the third or fourth magnitude, while many are equal to stars of the first, and some even surpass Jupiter and Venus in brilliancy. It is remarkable that the

largest are those which have the greatest altitudes, and only the smaller ones appear to come within 20 or even 40 miles of the earth.

With respect to the casual observations of the phænomena, the accounts of which are very numerous, the most interesting conclusion which has been inferred from them is the periodical recurrence of shooting-stars in unusual numbers at certain epochs of the year. Of these epochs, the most remarkable is that of November, on account of the prodigious number of meteors which have been seen in some years at that time. The principal displays were in 1799, 1832, 1833, and 1834. On the 11th of November, 1799, thousands were observed within a few hours by Humboldt and Bonpland at Cumana; and on the same night by different persons over the whole continent of America, from the borders of Brazil to Labrador, and also in Greenland and Germany. On November 12th, 1832, they were seen over the whole of the north of Europe; and on November 12th, 1833, the stupendous exhibition took place in North America which has been so often described. From the accounts of this phænomenon collected by Prof. Olmsted, M. Arago computed that the number of meteors on this night amounted to 240,000. In 1834, a similar phænomenon recurred on the night of November 13th, but on this occasion the meteors were of a smaller size. In 1835, 1836, and 1838, shooting-stars were observed on the night of November 13th, in different parts of the world; but though diligently looked for on the same night in the last few years, they do not appear to have been more numerous than on other nights about the same season,—a circumstance which has shaken the faith of many in their periodicity.

The second great meteoric epoch is the 10th of August, first pointed out by M. Quetelet; and although no displays similar to those of the November period have been witnessed on this night, there are more instances of the recurrence of the phænomena. In the last three years shooting-stars have been observed in great numbers, both on the 9th and 10th; but they appear in general to be unusually abundant during the two first weeks of August. The other periods which have been indicated are the 18th of October, the 23rd or 24th of April, the 6th and 7th of December, from the 15th to the 20th of June, and the 2nd of January; and it is not improbable that further observations will add to the number.

The different theories which have been given to explain the origin and phænomena of the shooting-stars are next stated. The following are the principal:—

1. That the shooting-stars and fire-balls are substances projected from volcanoes in the moon. It is known that a body projected vertically from the moon with a velocity of about 8500 feet in a second would not fall back upon the lunar surface, but would recede from it indefinitely; and, in order to reach the earth, the projectile would only require, under the most favourable circumstances, to have a velocity of about 8300 feet. Such a velocity, which is only about four or five times greater than that of a cannon-ball, is quite conceivable; but the extraordinary exhibitions of 1799 and 1833, to

say nothing of their supposed periodicity, is [are] utterly irreconcilable with the theory of a lunar origin. Benzenberg, however, adopts this theory, and supposes the shooting stars to be small masses of stone, from one to five feet in diameter, which are projected from lunar volcanoes, and circulate about the earth or about the sun when their projectile velocity exceeds a certain limit.

2. Dr. Olbers, and some other astronomers, have supposed the shooting-stars to be the *débris*, or fragments of a large planet, burst into pieces by some internal explosion, of which Ceres, Pallas, Juno, and Vesta, are the principal remaining portions. The smaller fragments continue to circulate about the sun in orbits of great eccentricity, and when they approach the region of space through which the earth is moving, they enter the atmosphere with great velocity, and by reason of the resistance and friction are rendered incandescent, and emit a vivid light so long as they remain within it.

3. It has been suggested by Biot that the extraordinary displays observed in November may be explained by supposing the meteors to have their origin in the zodiacal light. The extent of this lens-shaped nebulosity is not well ascertained; but as the plane of its principal section is not parallel to the ecliptic, if the earth passes through it at one season, it must be remote from it at another. But shooting-stars are observed at all times of the year; and the November meteors differ from those of other seasons in no respect excepting in their greater multitude.

4. The hypothesis first suggested by Chladni is that which appears to have met with most favour, having been adopted by Arago and other eminent astronomers of the present day to explain the November phænomena. It consists in supposing that, independently of the great planets, there exist in the planetary regions myriads of small bodies which circulate about the sun, generally in groups or zones, and that one of these zones intersects the ecliptic about the place through which the earth passes in November. The principal difficulties attending this theory are the following:—First, that bodies moving in groups in the circumstances supposed must necessarily move in the same direction, and consequently, when they become visible from the earth, would all appear to emanate from one point and move towards the opposite. Now although the observations seem to show that the predominating direction is from north-east to south-west, yet shooting-stars are observed on the same nights to emanate from all points of the heavens, and to move in all possible directions. Secondly, their average velocity (especially as determined by Wartmann) greatly exceeds that which any body circulating about the sun can have at the distance of the earth. Thirdly, from their appearance, and the luminous train which they generally leave behind them, and which often remains visible for several seconds, sometimes for whole minutes, and also from their being situated within the earth's shadow, and at heights far exceeding those at which the atmosphere can be supposed capable of supporting combustion, it is manifest that their light is not reflected from the sun; they must therefore be self-luminous, which is con-

trary to every analogy of the solar system. Fourthly, if masses of solid matter approached so near the earth as many of the shooting-stars do, some of them would inevitably be attracted to it; but of the thousands of shooting-stars which have been observed, there is no authenticated instance of any one having actually reached the earth. Fifthly, instead of the meteors being attracted to the earth, some of them are observed actually to rise upwards, and to describe orbits which are convex towards the earth; a circumstance, of which, on the present hypothesis, it seems difficult to give any rational explanation.

5. The most recent hypothesis is that of Capocci of Naples, who regards the aurora borealis, shooting-stars, aerolites, and comets, as having all the same origin, and as resulting from the aggregation of cosmical atoms, brought into union by magnetic attraction. He supposes that in the planetary spaces there exist bands or zones of nebulous particles, more or less fine, and endued with magnetic forces, which the earth traverses in its annual revolution; that the smallest and most impalpable of these particles are occasionally precipitated on the magnetic poles of our globe, and form polar auroras; that the particles a degree larger, in which the force of gravitation begins to be manifested, are attracted by the earth and appear as shooting-stars; that the particles in a more advanced state of concretion give rise in like manner to the phænomena of fire-balls, aerolites, &c.; that the comets, which are known to have very small masses, are nothing else than the largest of the aerolites, or rather, *uranolites*, which in course of time collect a sufficient quantity of matter to be visible from the earth. This theory of Capocci differs from Chladni's only by the introduction of magnetic forces among the particles, and it is obvious that all the objections to the former theory apply with equal force to this. It may be remarked, however, that some physical connexion between the phænomena of shooting-stars and aurora had been already suspected, and the observations adduced by M. Quetelet afford reason to suppose that the latter phænomenon is also periodical.

From the difficulties attending every hypothesis which has hitherto been proposed, it may be inferred how very little real knowledge has yet been obtained respecting the nature of the shooting-stars. It is certain that they appear at great altitudes above the earth, and that they move with prodigious velocity; but everything else respecting them is involved in profound mystery. From the whole of the facts M. Wartmann thinks that the most rational conclusion we can adopt is, that the meteors probably owe their origin to the disengagement of electricity, or of some analogous matter, which takes place in the celestial regions on every occasion in which the conditions necessary for the production of the phænomena are renewed.

The concluding part of the paper contains an account of the different attempts which have been made to deduce differences of longitude from the observation of shooting-stars. That meteors which appear and are extinguished so suddenly, and which by reason of

their great altitude and brilliancy are visible over considerable portions of the earth's surface, would afford excellent natural signals, provided they could be identified with certainty, was an obvious thought; but so long as they were regarded merely as casual phænomena, it could scarcely be hoped that they would be of much use, in this respect, to practical astronomy. As soon, however, as their periodicity became probable, the observation of the phænomena acquired a new interest. In observing the meteors for this purpose, it is assumed that they appear instantaneously to observers stationed at a distance from each other, and that the meteors seen by different observers so placed are identically the same. These points are not altogether free from uncertainty; but the results of the trials that have been already made may be regarded as favourable, and as showing that among the other methods of determining astronomical positions, the observation of shooting-stars is not to be disregarded. At the November meeting of this Society, in 1839, an account was given of Professor Schumacher's observations at Altona on the night of the 10th of August, 1838. On the same night, corresponding observations were made at several observatories in Germany; but those at Breslaw appear to have been the most successful. From twelve coincident observations at Altona and Breslaw, Professor Boguslawski computed the difference of longitude of the two places to be $28^{m}\ 22^{s}{\cdot}07$, which differs less than a second from that which had been previously adopted. In Silliman's American Journal for October 1840, an account is given of simultaneous observations made on the 25th of November, 1835, at Philadelphia, and at the College of New Jersey, at Princeton. Seven coincidences were observed, and the mean result gave a longitude differing only $1^{s}{\cdot}2$ from the mean of other determinations; the whole difference being two minutes. This appears to have been the first actual determination of a difference of longitude by meteoric observations. In the corresponding observations of Wartmann and Reynier at Geneva and Planchettes, the differences of longitude deduced from three of the meteors, which were attended with peculiarities so remarkable as to leave no doubt of their identity, were respectively 2^{m}, $2^{m}\ 3^{s}$, $2^{m}\ 5^{s}$, whence it would seem that a single observation may be in error to the amount of several seconds of time. In the *Bibliothèque Universelle de Genève* for August 1840, there is given an account of the determination by this method of the difference of longitude between Rome and Naples. The corresponding observations were begun in November 1838, and were continued at intervals under the direction of Father Vico at Rome, and of Capocci and Nobili at Naples. The apparent paths of the meteors were traced on a celestial globe, and the times of appearance and extinction compared with clocks regulated by astronomical observations. The observed times of the extinction of the phænomena presented a very satisfactory agreement, inasmuch as it is stated that there was in general a difference of only a few tenths of a second of time between the partial results for a difference of longitude amounting to $7^{m}\ 5^{s}{\cdot}7$.

The merit of first suggesting the use of shooting-stars and fire-

balls as signals for the determination of longitudes is claimed by Dr. Olbers and the German astronomers for Benzenberg, who published a work on the subject in 1802. Mr. Baily, however, has pointed out a paper published by Dr. Maskelyne twenty years previously, in which that illustrious astronomer calls attention to the subject, and distinctly points out this application of the phænomena. The paper, which is printed on a single sheet, is entitled "A Plan for observing the Meteors called Fire-balls, by Nevil Maskelyne, D.D., F.R.S., and Astronomer Royal," and is dated Greenwich, November 6th, 1783. After recounting some observations, from which he infers that such meteors appear more frequently than is commonly imagined, and stating the particulars to be attended to in observing them, he adds:—

"It would be well if those persons who happen to see a meteor would put down the time by their watch when it first appeared, or was at its greatest altitude, or burst, or disappeared, and again when they hear the sound; and as common watches are liable to vary much in a few hours, that they would, as soon after as may be, find the error of their watch by a good regulator; for, if *the exact time could be had at different places,* the absolute velocity of the meteor, *the velocity of the sound propagated to us from the higher regions of the atmosphere, and the longitudes of places, might be determined.*"

Extracts from the Report of the Council of the Society to the Twenty-first General Annual Meeting, held February 1, 1841.

"The recurrence of this annual meeting of the Society affords the Council its usual opportunity of referring to the principal events of the past session. These, although few in number, are full of interest to those who prosecute the varied branches of astronomical science. For, since our last anniversary, a comet has been discovered at Berlin, by that indefatigable astronomer, M. Galle, which appears to be the same that was observed in the year 1097, and again in the year 1468; thus performing its revolution in about 371 years. And, more recently, another comet has been discovered also at Berlin, by M. Bremicker; which is now visible to those who are possessed of a powerful telescope, but which, however, from the elements at present deduced, has never been previously seen by any human being. The Council cannot but congratulate the Society that this branch of the science appears now more likely to be prosecuted than it has recently been, and thus lead to discoveries that may eventually have an important bearing on our knowledge of the laws and physical constitution of the universe. The subject of parallax also has engaged more than ordinary attention within the last year from the interest excited by M. Bessel's valuable observations on the double star 61 Cygni, which now appear to be brought to a close, and which the Council have considered of sufficient importance to entitle him to the award of the medal of this Society, as will be fully explained by the President in his address at the close of this meeting."

Amongst the instruments alluded to in the statement of the property of the Society, "the Council have the satisfaction of bringing before the notice of the members, the valuable joint present, from Sir John Herschel and his aunt Miss Caroline Herschel, of a seven-feet reflecting telescope made by the late Sir William Herschel, and used by her in many of her observations. This token of respect and attention will be duly estimated by the Society, who will doubtless preserve this interesting memorial of science with more than ordinary care.

"Amongst the losses by death, during the past year, the Council have to regret the decease of Capt. Drummond, Dr. Gregory, Prof. Leybourn, and Mr. Best, on the home list; and MM. Olbers, Poisson, and Littrow, on the foreign list.

"Capt. Thomas Drummond was born at Edinburgh in October 1797, and entered the corps of Royal Engineers in July 1815. In this department his talent for mechanical combinations became conspicuous, which, together with his close attention to the study of chemistry, rendered his services of considerable value. In 1819, he took part in the trigonometrical survey which was then carrying on in Great Britain, under the superintendence of Col. Colby: and whilst in this employment, he suggested the happy idea of applying the brilliant lamp, which goes under his name, to rendering visible the distant stations. An account of this valuable invention is printed in the Philosophical Transactions for 1826*.

"Capt. Drummond was an early member of this Society, and took great interest in promoting its welfare. During the comparisons of the standard scale of the Society with the parliamentary standard, he frequently attended the meetings of the Committee, and aided them with his opinion and advice on several important points. His previous knowledge and experience on such subjects had been well matured by the laborious and extensive comparisons of the new standard bars, used in the trigonometrical survey, that had just before been carried on at the Tower, principally under his management; and where he had alternately to encounter the heat of an oven and the cold of an ice-house: no method being left untried for attaining the greatest degree of accuracy, both in the measures themselves, and in determining the rate of expansion.

"It is supposed that the first shock which Capt. Drummond's constitution received, was in laying down the base-line at Loch Foyle, in Ireland; where he was oftentimes exposed to the inclemencies of the weather, and himself frequently standing in deep water, earnestly intent on the operations that were then carrying on, as a foundation for the future survey of the country. In fact, he entered with so much ardour and zeal into whatever he undertook, that he might be considered the life and soul of every enterprise in which he was engaged. But he was withdrawn from his geodesical pursuits by the Lord Chancellor (Brougham), who placed him at the head of the Boundary Commission that was established as a preliminary to the

[* See Phil. Mag., First Series, vol. lxvii. p. 373. 453.—Ed.]

Reform Bill. In this new employment Capt. Drummond exerted himself with his usual zeal and ability, which led to a more intimate connexion with the ministers then in power; and he was, in April 1832, appointed private secretary to Lord Althorp, then Chancellor of the Exchequer. In this situation he aided the cause of this Society, and was the principal means of our obtaining the grant of the present apartments which we now occupy. In July 1835, he was appointed under-secretary to the Earl of Mulgrave, Lord Lieutenant of Ireland. The arduous duties of his office, united with other employments which he undertook, more especially as first commissioner of the Irish Railway Commission, in which his usual good sense and indefatigable exertions were manifest, proved too much for his physical strength. His constitution gradually gave way, and it was soon apparent that the scene was about to close upon him for ever. At length, 'in the plentitude of mental power and the maturity of knowledge, beloved in private and esteemed in public life,' he expired on the 15th of April last, and was followed to the grave by thousands who revered his memory and mourned his loss.

"Dr. Olinthus Gregory was one of the earliest members of the Society, and for some time held the office of secretary. Though educated in the old English school of mathematics, his acquaintance with the continental methods was much more extensive than would have appeared from his writings, which were almost altogether intended for those who had studied the older English writers. He knew generally what was going on abroad, particularly in the extensions of geometry made by the school of Monge. As editor of the well-known Ladies' and Gentleman's Diaries, he was brought into communication with young students who were desirous of distinguishing themselves in the exact sciences. The protection and encouragement which he afforded to those who were pursuing the path which he himself had trodden, will be gratefully remembered by many; and the period of his superintendence of those useful works will be remembered as that in which every contributor of merit found a friend in the editor. His manners were altogether in accordance with what might have been expected from the preceding account; all he did and said was dictated by benevolence of feeling.

"Dr. Gregory's occupations were numerous and engrossing, and his power of application was very great. About thirteen years ago he was attacked with severe illness, from which it was hardly expected that he would finally recover. Although more or less of an invalid from that time till his death, he continued his numerous avocations with all but, if not altogether, his accustomed energy. On the removal of the tax on almanacs, he was the first to see that the publications which he superintended might be made still more useful in diffusing the spirit of scientific inquiry; and, from that time till his death, original treatises, or useful reprints, were made to form part of them.

"The writing by which he is best known to the public at large is his letters on the evidences of Christianity, a work of large circulation. His treatise on mechanics had a considerable celebrity, and

was translated into German; it is not improbable that some here present may have learned their first ideas of that science from it. His edition of Dr. Hutton's Course of Mathematics was considerably augmented by himself, and is the best of them all. As an accurate observer, he is known by his experiments on the velocity of sound, which agree almost exactly with those of the first French and three Dutch observers, who were almost simultaneously employed on the same subject*.

"We have frequently to record losses which bear more directly on practical astronomy, but it is not often that we have to regret the termination of a more energetically useful career, and never of a more zealous one. The principal works known to have been written or edited by him are as follow:—

1793. Lessons, Astronomical and Philosophical, 1 vol.
1801. Treatise on Astronomy, 1 vol.
1802. The Gentleman's Diary, under his editorship.
1806. Treatise on Mechanics, 3 vols.
1807. Translation of Haüy's Natural Philosophy, 2 vols.
1808. Pantalogia, of which he was the general editor, and the contributor of about one half, 12 vols.
1810. Third volume of Dr. Hutton's Course of Mathematics, of which he wrote about one half; and he afterwards edited an edition of the whole course.
—— Letters on the Evidences, &c. of Christianity, 2 vols.
1815. Tracts on the Trigonometrical Survey.
1816. Plain and Spherical Trigonometry, 1 vol.
—— Dissertation on Weights and Measures.
1817. An account of his Pendulum Experiments and Astronomical Observations made at Shetland, in the Philosophical Magazine [First Series, vol. liii. p. 426.].
1818. Appointed editor of the Ladies' Diary and general superintendent of the Stationers' Company's Almanacs.
1825. Mathematics for practical Men, 1 vol.
1839. Address to the Cadets of the Royal Military Academy on resigning the Professor's Chair.
1840. Hints to Mathematical Teachers, 1 vol.
—— Tables to be used with the Nautical Almanac.

"He was also, at one period of his life, a large contributor to the leading reviews.

"Thomas Leybourn, Esq. Fellow of the Royal Society, late senior Professor of Mathematics in the Royal Military College, was one of the original members of this Society, having been admitted by the founders in 1820. Mr. Leybourn was born in 1769, at Bishop-Middleham, in the county of Durham. Although he had not the advantages of a regular education, he appears to have devoted himself to mathematical studies from an early period of life; for, soon after 1790, he was a correspondent of Dr. Hutton in the Ladies' Diary; and, in 1795, he became known to the public as the editor

[* Dr. Gregory's paper on this subject will be found in Phil. Mag., vol. lxiii. p. 401.—Ed.].

of the Mathematical Repository —a publication which made its appearance at irregular intervals, and of which the object was to afford a channel for giving publicity to lucubrations of greater length, and to the solutions of problems of a higher order of difficulty, than could be admitted into the Diaries. At that time he was employed as land-agent on the estate of a nobleman in Wales. In 1802 he was appointed, on the recommendation of Dr. Hutton, one of the mathematical instructors in the Military College then recently established at Great Marlow—an office which he continued to fill with much ability and advantage to the institution, until within a few months before his death, which took place in March 1840.

"The first series of the Mathematical Repository terminated in 1802, when the work had extended to five duodecimo volumes. Another series was begun in 1803, which was continued, though with some long intermissions, until 1835, when it had extended to six volumes 8vo. To this publication some of the most distinguished mathematicians of the country were occasional contributors; and although, as in all other periodicals, the contributions are of very unequal merit, the work is on the whole valuable to the student of geometry, particularly the earlier volumes of it, which contain many very beautiful specimens of the application of the ancient geometrical analysis. Besides the Mathematical Repository, Mr. Leybourn was editor of the 'Questions proposed in the Ladies' Diary,' published in 1817, in four vols. 8vo; and of the Gentleman's Diary from 1824 till his death. To these several works he contributed little of his own; but his merit in bringing them forward, and the liberality he showed in defraying the expense of their publication (for, with the exception of the Gentleman's Diary, they were all attended with a considerable pecuniary sacrifice), will secure for his name the respect of those who wish well to the propagation of useful knowledge.

"Mr. Leybourn's distinguishing merit, however, consisted in the ability and zeal with which, during the long period of thirty-seven years, he discharged his laborious public duties in the Military College. As an instructor he was highly respected and esteemed by the governors of the institution, and much beloved by his pupils, in whose progress and advancement he took the warmest interest. He was a man of a kind and benevolent disposition; ready at all times to extend a helping hand to those who stood in need of assistance, more especially if they came recommended to him as possessing a taste for the mathematics.

"Richard Best, Esq. was one of the earliest members of this Society, and, while his health permitted, he was a regular attendant at the monthly meetings. He had a sort of hereditary connexion with our science through his mother, who was a niece of Dr. Bradley, being the daughter of his sister Rebekah, the wife of Mr. John Dallaway; and he possessed some books and instruments which had once been the property of that distinguished astronomer. The popular branches of astronomy were to him a source of amusement; he was a diligent observer of telescopic phænomena, and he occa-

sionally furnished an account of them to the periodicals of the day. He was greatly respected in his own neighbourhood, having long been treasurer to most of the local charities, and a liberal supporter of them all. He was a native of Greenwich, where he resided till within a few months of his death; and, for the last thirty-five years, lived in a house which was built by his grandfather, and occupied by him for the first time on the day his son, Mr. Best's father, came of age.

"Mr. Best retired to Henley-on-Thames, where he died on the morning of May 19, 1840, the day on which he would have completed his seventy-third year, regretted by all who had the pleasure of his acquaintance.

"The important discovery of two new planets in our system (Pallas and Vesta) has rendered the name of Olbers familiar to every lover of astronomy. The circumstances that led to this discovery were as singular as they were fortunate; and show the happy results that may arise from a zealous association of individuals in the steady pursuit of some definite object. The detail of those circumstances has been so recently given in the obituary of this distinguished astronomer and mathematician, read at the last anniversary meeting of the Royal Society, that it appears almost needless to repeat them in this place. Yet it may here be stated, that the discovery of Pallas at nearly the same distance from the sun as Ceres (which had been discovered in the preceding year) led Olbers to conjecture that they were fragments of a larger planet, which might have been scattered by some great catastrophe, and that, probably, some other portions of the original mass might be found in nearly the same orbit. His diligence was rewarded by the discovery of Vesta, about five years afterwards; and nearly in the position in which he expected it would be found. The instrument with which he made these discoveries was a very small telescope, and his observatory was a room in the upper part of his house; thus showing to the world with what slender means the most important results may be obtained. But Olbers was not merely a practical astronomer; his treatise on the best mode of determining the orbit of a comet, and his improvements and investigations of various astronomical formulæ, exhibit him as a mathematician of considerable talent. One of the latest papers relative to astronomy, on which he was employed prior to his death, was on a reform of the constellations, both as to their nomenclature and the arrangement of the stars which limit their boundaries: to which subject he was excited by a passage in the Report of the Council at our last anniversary, wherein it was stated that a revision of this kind was about to be made under the superintendence of a committee appointed by the British Association. The views of Olbers are in perfect coincidence with the object proposed; and in the paper above alluded to, and which has been transmitted to the President, he laments the confusion that has been introduced by his predecessors, and suggests some useful hints for a remedy *.

[* For another obituary notice of Dr. Olbers, see Phil. Mag., S. 3., vol. xviii. p. 72.—Ed.]

"Simeon Denis Poisson (born June 21, 1781, died April 25, 1840) was placed, by common consent, at the head of European analysts on the death of Laplace. He was of humble birth, and was admitted in 1793 a student of the *Ecole Polytechnique*, then newly established. It is stated, by the historian of this school, that, at the age of eighteen, he submitted to his professor some ameliorations in the method of demonstrating the binomial theorem; which that teacher, who was no other than Lagrange, read publicly at his next lecture, and which he declared his intention of adopting in future.

"The life of Poisson was one of quiet and uninterrupted study. He never held any situation connected with politics, nor was in any way, during thirty years, prevented from pursuing his one great object, the application of the most abstruse and newest developments of the integral calculus to problems of physics. The number of his memoirs is enormous; to which must be added his elementary treatise on mechanics (which stands at the head of all elementary writings on the application of pure analysis to the properties of matter), his treatises on capillary attraction, on heat*, and on the theory of probabilities.

"It is well known that the energies of Euler, Clairaut, D'Alembert, and the younger Bernoullis, had organised the application of mathematics in a manner which made the subsequent triumphs of Lagrange and Laplace seem almost beyond expectation. The power of the pure mathematics seemed to flag, when Fourier first came forward with his applications of definite integrals and periodic series to questions of physics, which seemed to be unconquerable, and of which the difficulties seemed to be altogether inexpressible by ordinary analysis. A new school of mathematicians was rapidly formed, in whose hands the mode of expression by definite integrals added one more to the instances in which the happy enunciation of questions was all but their solution. Poisson was one of the first of this school in point of time, and by far the greatest in power. Throughout the major part of his writings we trace the same capability of explaining the most abstruse points with fluent clearness and rigid accuracy, combined with that of conquering the physical difficulties of his problem by the most happy art of adaptation.

"Many of his memoirs are on the great questions of physical astronomy, and it is here that he shows that he was not the accident of a fortunate epoch, but that he could handle the instruments of his two great predecessors with skill resembling their own. Perhaps his greatest achievement in this line is the extension of our knowledge respecting the stability of the solar system, as far as it may be affected by perturbations of the mean orbital motions, or of the axical rotations. This question does not, as many imagine, owe all its interest either to the predictive power which is sought, or to the grandeur of the problem considered as the path to such a power. It is to be remembered that the connecting constants between the oldest and most recent astronomy are the lengths of the sidereal year and of the day; and that we cannot assume to talk a common lan-

[* See Taylor's Scientific Memoirs, vol. i. p. 122.—Ed.]

guage with Hipparchus and Ptolemy, unless we have reason to know that these elements continue sensibly unaltered. In addition to the imperfect presumptions derived from observation (imperfect on account of the large liability to error of the older astronomers) Lagrange had shown that the mean motions have no secular inequalities depending on the first power of the disturbing forces; or, so far as this first power was concerned, on any powers of the eccentricities or inclinations. Laplace had shown that a certain secular equation, which should in theory be applied to the sidereal day, would always be too small to be of any importance. Poisson extended the conclusions of Lagrange to the second power of the disturbing forces, and, relatively, to any powers of the eccentricities and inclinations; or rather, we may say, that he showed any secular equation of the mean motions to depend only on the fourth power of disturbing forces; for, in the course of the investigation, it appears that no such equation of any odd order can exist. As far as the fourth powers of eccentricities and inclinations, he actually shows the mutual destruction of an infinite number of non-periodic disturbing terms; the rest of the powers are completed by a general and different investigation. In the problem of the rotation of the earth, he generalises the investigation of Laplace, by taking into consideration the actual change of the axis *on the earth*; the former investigation considering only the change of the axis, supposed to be fixed in the earth, relatively to the stars. The result agrees with that of Laplace as to the non-existence of any sensible secular inequality.

"Poisson belongs to a class of investigators of whom many are always wanted, but one is permanently indispensable.

"The applications of the newest powers of mathematical language should always be made as near as may be to the time when they are first exhibited. But such peculiar combination of address and power, with a perfect knowledge of existing materials, is not of every-day occurrence; nor is it permitted to say, *uno avulso non deficit alter**.

"His Majesty the King of Denmark, who was elected an honorary member of this Society at the last anniversary, has been pleased to continue the offer of the gold medal founded by his royal predecessor, for the first discovery of a telescopic comet; subject to the conditions and regulations already inserted in the Monthly Notices of this Society for November 1835 †. One of these conditions (which is indispensable as far as it affects persons resident in Great Britain) is, that notice of such discovery must be sent by the *first* post to Mr. Baily.

"The Council cannot conclude this Report without expressing a hope that the future efforts of the Society for the promotion and encouragement of astronomy, will be marked with the same success as has hitherto attended their career: and that they may continue to show to the world the happy effects of unanimity and zeal in the pursuit of a favourite science."

[* Another obituary notice of M. Poisson will be found at p. 74 of the preceding volume.—EDIT.]

[† See Phil. Mag., Third Series, vol. ix. p. 294.—ED.].

The President (Sir J. F. W. Herschel, Bart.) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—The Report of the Council has placed before you so ample a view of the state of the Society, of its labours during the last year, of the accessions to its members, and of the many and severe losses it has had to deplore, that little is left for me to add, except my congratulations on its continued and increasing prosperity. It would be inexpressibly gratifying to me if I could persuade myself that my own exertions in its Chair had contributed, even in a small degree, to that prosperity; but, alas! I have felt only too sensibly how very feebly and inefficiently, especially during the last year, owing to a variety of causes, but chiefly to residence at a distance from London, I have been able to fill that most honourable office.

The immediate object of my now addressing you, gentlemen, is to declare the award by your Council of the gold medal of this Society to our eminent associate, M. Bessel, for his researches on the annual parallax of that remarkable double star 61 Cygni *, researches which it is the opinion of your Council have gone so far to establish the existence and to measure the quantity of a periodical fluctuation, annual in its period and identical in its law with parallax, as to leave no reasonable ground for doubt as to the reality of such fluctuation, as something different from mere instrumental or observational error: an inequality, in short, which, if it be *not* parallax, is so inseparably mixed up with that effect, as to leave us without any criterion by which to distinguish them. Now, in such a case, parallax stands to us in the nature of a *vera causa,* and the rules of philosophizing will not justify us in referring the observed effect to an unknown and, so far as we can see, an inconceivable cause, when this is at hand, ready to account for the whole effect.

I say, in the nature of a *vera causa,* since each particular star must of necessity have some parallax. Every *real, existing material* body, must enjoy that indefeasible attribute of body, viz. *definite place.* Now place is defined by *direction* and *distance* from a fixed point. Every body, therefore, which does exist, exists at a certain definite distance from us and at no other, either more or less. The distance of every individual *body* in the universe from us is, therefore, necessarily admitted to be finite.

But though the distance of each particular star be not in strictness infinite, it is yet a real and immense accession to our knowledge to have *measured* it in any one case. To accomplish this, has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct

[* See Phil. Mag., Third Series, vol. xiv. p. 226.—Ed.].

apprehension, and so leading him on in hopeless, endless, and exhausting pursuit.

The pursuit, however, though eager and laborious, has been far from unproductive even in those stages where its immediate object has been baffled.

The fact of a periodical fluctuation of *some kind* in the apparent places of the stars was recognized by Flamsteed, and erroneously attributed to parallax. The nearer examination of this phænomenon with far more delicate instruments, infinitely greater refinement of method, and clearer views of the geometrical relations of the subject, rewarded Bradley with his grand discoveries of aberration and nutation, and enabled him to restrict the amount of possible parallax of the stars observed by him within extremely narrow limits.

Bradley failed to detect any appreciable parallax, though he considered 1″ as an amount which would not have escaped his notice. And since his time this quantity has been assumed as a kind of conventional limit, which it might be expected to attain but hardly to surpass. But this was rather because, in the best observations from Bradley's time forward, 1″ has been a tolerated error; a quantity for which observation and mechanism, joined to atmospheric fluctuations and uncertainties of reduction, could not be held rigidly accountable even in mean results; than from any reason in the nature of the case, or any distinct perception of its reality. If parallax were to be detected at all by observations of the absolute places of the stars, it could only emerge as a "residual phænomenon," after clearing away all the effects of the uranographical corrections as well as of refraction, when it would remain mixed up with whatever uncertainties might remain as to the coefficients of the former, with the casual irregularities of the latter, and with all the forms of instrumental and observational error. Now these have hitherto proved sufficient, even in the observation of zenith stars, quite to overlay and conceal that minute quantity of which astronomers were in search.

It is not my intention, gentlemen, to enter minutely into the history of the attempts of various astronomers on this problem, whether by the discussion of observations of one star, or by the combination of those of pairs of stars opposite in right ascension; nor with the occasional gleams of apparent success which, however, have always proved illusory, which have attended these attempts. For such a history, and, indeed, for a complete and admirably drawn up monograph of the whole subject, I must refer to a paper lately read to this Society by Mr. Main, and which is now in process of publication in the forthcoming volume of our Memoirs *. In whatever reference I may have to make to the history of the subject, I must take this opportunity to acknowledge my obligations to the author of this paper, as well as for his exceedingly luminous exposition of the results of those more successful attempts on the

[* See Phil. Mag., Third Series, vol. xviii. p. 597.—Ed.].

problem by Henderson, Struve, and Bessel, which I shall now proceed more especially to consider.

It would be wrong, however, not to notice that the first indication of some degree of impression beginning to be made on the problem seems to be found in Struve's discussion of the differences of right ascension of circumpolar stars in 1819, 20, and 21. The only *positive* result, indeed, of these observations is, that in the case of twenty-seven stars examined, none has a parallax amounting to half a second. But *below* this, there certainly do seem to be indications in the nature of a real parallax, which might at least suffice to raise the sinking hopes of astronomers, and excite them to further efforts.

But the time arrived when the problem was to be attacked from a quarter offering far greater advantages, and exposed to few or none of those unmanageable sources of irregular error to which the determinations of absolute places are liable. I mean by the measurement of the distances of such double stars as consist of individuals so different in magnitude as to authorize a belief of their being placed at very different distances from the eye; or, as Struve expresses it, *optically* and not *physically* double. This, in fact, was the original notion which led to the micrometrical measurements of double stars; but not only was anything like a fair trial of the method precluded by the imperfections of all the micrometers in use until recently, but the interesting phænomena of another kind, which began to unfold themselves in the progress of those measurements, led attention off altogether from this their original application, which thus lay dormant and neglected, until the capital modern improvements, both in the optical and mechanical parts of refracting telescopes, and the great precision which it was found practicable, by their aid, to attain in these delicate measurements, revived the idea of giving this method, what it never before had, a fair trial. The principle on which the determination of parallax by means of micrometrical observations of a double star turns, is extremely simple. If we conceive two stars very nearly in a line with the eye, but of which one is vastly more remote than the other, each, by the effect of parallax, will appear to describe annually a small ellipse about the mean place as its centre. These two ellipses, however, though similar in form will differ in dimension; that described by the more remote star being comparatively much smaller: consequently, the apparent places being similarly situated in each, their apparent distance on the line joining these apparent places will both oscillate in angular position and fluctuate in length, thus giving rise to an annual relative alternate movement between the individuals both in position and distance, which is greater the greater the difference of the parallaxes.

Thus it is not the absolute parallax of either, but the difference of their parallaxes, which is effectively measured by this method; *i. e.* by repeating the measurements of their mutual distance at all times of the year. But, on the other hand, aberration, nutation,

precession, and refraction, act equally on both stars, or so very nearly so as to leave only an exceedingly small fraction of these corrections bearing on the results. And when the stars are very unequal in magnitude, there is a presumption that the difference of their parallaxes is very nearly equal to the whole parallax of the nearer one.

The selection of a star for observation involves many considerations. In that pitched on by M. Bessel (61 Cygni), the *large star* so designated, is in fact a fine double star; nay, one that has been ascertained to be physically double. It is in every respect a highly remarkable star. The mutual distance of its individuals is great, being about $16\frac{1}{4}''$. Now this being necessarily less than the axis of their mutual orbit, affords in itself a presumption that the star is a *near one.* And this presumption is increased by the unusually great proper motion of this binary system, which amounts to nearly $5''$ per annum, and which has been made by Sir James South the subject of particular inquiry, and found to be *not* participated in by several small surrounding stars, *which, therefore,* are not physically connected with it. Moreover, the angular rotation of the two, one about the other, has been well ascertained.

Now, it fortunately happens, that of these small surrounding stars there are two very advantageously situated for micrometrical comparison with either of the individuals of the binary star, or with the middle point between them. The one of these (a), at a distance of $7' 42''$, is situated nearly at right angles to the direction of the double star; the other (b) at a distance of $11' 46''$, nearly *in* that direction. Considering (a) and (b) as fixed points then, and measuring at any instant of time their distances from (c), the middle point of the double star, the situation of (c) relative to (a) and (b) is ascertained; and if this be done at every instant, the relative *locus* of (c), or the curve described by it on the plane of the heaven with respect to the fixed base-line $a\,b$, will become known.

Now, on the hypothesis of parallax, that locus ought to be an ellipse of one certain calculable eccentricity and no other. And its major and minor axes ought to hold with respect to the points, a, b, certain calculable positions and no other. Hence it follows that the distances $a\,c$ and $b\,c$ will each of them be subject to annual increase and diminution; and *that,* 1st, in a given and calculable ratio the one to the other; and 2dly, so that the maxima and minima of the one distance ($a\,c$) shall be nearly contemporaneous with the *mean* values of the other distance $b\,c$, and *vice versâ.*

Thus we have, in the first place, several particulars independent of mere numerical magnitudes; and, in the second place, several distinct relations *à priori* determined, to which those numerical values must conform, if it be true that any observed fluctuations in these distances ($a\,b$) ($a\,c$) be really parallactic. So that if they be found in such conformity, and the above-mentioned maxima and minima do observe that interchangeable law above-stated; and if, moreover, all due care be proved to have been taken to eliminate every instrumental source of annual fluctuation; there becomes ac-

cumulated a body of probability in favour of the resulting parallax, which cannot but impress every reasonable mind with a strong degree of belief and conviction.

Now, all these circumstances have been found by M. Bessel, in his discussion of the measures taken by him (which have been very carefully and rigorously examined by Mr. Main in the paper alluded to, as have also M. Bessel's formulæ and calculations, for in such matters nothing must remain unverified), to prevail in a very signal and satisfactory manner. Not one case of discordance, in so many independent particulars, has been found to subsist; and this, of itself, is high ground of probability. But we may go much further. Mr. Main has projected graphically the deviations of the distances (a c) and (b c) from their mean quantities (after clearing them of the effects of proper motion and of the minute differences of aberration, &c.). Taking the time for an abscissa, and laying down the deviations in the distances so cleared as ordinates, two curves are obtained, the one for the star (a), the other for the star (b). Each of these curves ought alternately to lie for half a year above, and for half a year below, its axis.—*It does so.* Each of them ought to intersect its axis at those dates when the maximum and minimum of the other above and below the axis occurs. With only a slight degree of hesitation at one crossing—*it does so.* The points of intersection with the axis ought to occur at dates in like manner calculated *à priori*; and so they do within very negligible limits of error. And, lastly, the general forms, magnitudes, and flexures of the curves ought to be identical with those of curves similarly projected, by calculation on an assumed resulting parallactic coefficient. This is the final and severe test: Mr. Main has applied it, and the results have been placed before you:—*oculis subjecta fidelibus.* If all this does not carry conviction along with it, it seems difficult to say what ought to do so.

The only thing that can possibly be cavilled at is the shortness of the period embraced by the observations; viz. from August 1837 to the end of March 1840. But this interval admits of five intersections of each curve with its axis; of two maxima and two minima in its excursions on either side; and of ample room for trying its agreement in general form with the true parallactic curves. Under such circumstances, it is quite out of the question to declare the whole phænomenon an accident or an illusion. *Something* has assuredly been discovered, and if that something be not parallax, we are altogether at fault, and know not what other cause to ascribe it to.

The instrument with which Bessel made these most remarkable observations is a heliometer of large dimensions, and with an exquisite object-glass by Fraunhofer. I well remember to have seen this object-glass at Munich before it was cut, and to have been not a little amazed at the boldness of the maker who would devote a glass, which at that time would have been considered in England almost invaluable, to so hazardous an operation. Little did I then imagine the noble purpose it was destined to accomplish. By the

nature and construction of this instrument, especially when driven by clock-work, almost every conceivable error which can affect a micrometrical measure is destroyed, when properly used; and the precautions taken by M. Bessel in its use have been such as might be expected from his consummate skill. The only possible apparent opening for an annually fluctuating error seems to be in the correction for temperature of its scale. But this correction has been ascertained by M. Bessel by direct observation, in hot and cold seasons, and applied. Nor could this cause destroy the evidence arising from the simultaneous observation of the two companion stars, since a wrong correction for temperature would affect both their distances proportionally, leaving the apparent parallactic movement still unaccounted for.

The resulting parallax is an extremely minute quantity, only thirty-one hundredths of a second; which would place the star in question at a distance from us of nearly 670,000 times that of the sun!* Such is the universe in which we exist, and, which we have at length found the means to subject to measurement, at least in one of its members, probably nearer to us than the rest.

It becomes necessary for me now to refer to two series of researches on this important subject, which have been held by your Council to merit very high and honourable mention; though neither of them, separately, for reasons which I shall state, would have been considered as carrying that weight of probability in favour of its conclusions, which would justify any immediate decision of the nature which they have come to in the case of M. Bessel's. I allude to M. Struve's inquiries, by the method of micrometric measures, into the parallax of α Lyræ; and to Mr. Henderson's, by that of meridian observations, on the parallax of α Centauri.

α Lyræ is accompanied by a very minute star, at the distance of about 43″. That this star is unconnected with α by any physical relation, is clear from the fact ascertained by Sir James South and myself, that it does not participate in the proper motion of the large star. The mutual angular distance of these stars has been made by M. Struve the subject of a very extensive series of micrometric measures with the celebrated Dorpat achromatic, bearing this object steadily in view, and working it out to a conclusion of the very same kind, and, though materially inferior in the degree and nature of its evidence to that of Bessel, yet certainly entitled to high consideration. M. Struve's observations on this star, and for this purpose, extend from Nov. 1835 to Aug. 1838, and are distributed over sixty nights, averaging twenty per annum; and from their combination, according to the principle of probabilities, he concludes a parallax of 0″·261. Mr. Main has subjected these observations to an analysis and graphical projection, precisely similar in principle to those I have explained in the case of 61 Cygni. The curves so projected have been subjected to your inspection, and

* The orbit described by the two stars of 61 Cygni about each other will, therefore, be about 50 times the diameter of the earth's about the sun, or $2\frac{1}{2}$ times that of Uranus.

that inspection certainly does leave a very strong impression of a real and tolerably well-ascertained parallax *having* been detected in this star. But at the same time an impression no less decided, owing to irregularities in the march of the curve, when compared with the true parallactic curve, is created,—that the errors of observation are far from being eliminated,—that, on the contrary, they bear such a proportion to the parallax itself as to leave room for some degree of hesitation, and to justify an appeal to a longer series of observations, and to concurrent evidence from other quarters, before declaring any positive opinion. The evidence of this kind, in short, is not equal to that afforded by the similar projection of Bessel's observations of *either* of his two comparison stars. And to this it must be added, that only one star of comparison existing in the line of α Lyræ, the possible effect of temperature and *annual* instrumental variation is not eliminated from the result in the way in which it is from the measures of 61 Cygni; while all that great mutual support which the observations of parallaxes of the two comparison stars afford each other in the latter case, is altogether wanting in the former. These considerations, without any under-estimation of the great importance and value of M. Struve's researches, yet formed essential drawbacks on the immediate admission of his results.

In a word, I conceive the question of discovery as between these illustrious, but most generous and amicable rivals, may be thus fairly stated. M. Struve's meridian observations in 1819–1821 seem to have made the first impression on the general problem, but too slight to authorize more than a hope that it would yield at no distant day. His micrometric measures of α Lyræ commenced more than a year earlier, and have extended altogether over a longer period than M. Bessel's of 61 Cygni. From their commencement they afford indications of parallax, and these indications accumulating with time have amounted to a high degree of probability, and rendered the supposition of parallax more admissible than that of instrumental or casual errors producing the same influence on the measures. On the other hand, M. Bessel's measures commencing a year later, and continued on the whole through somewhat less time, have exhibited a compact and consistent body of evidence drawn from two distinct systems of measures mutually supporting each other, and so steadily bearing on their object as to leave no more reasonable doubt of its truth than in the case of many things which we look upon as, humanly speaking, certain. And this conviction once obtained, reacts on our belief in the other results, and induces us to receive and admit it on the evidence adduced for it; which, without such conviction so obtained, we might hesitate to do until after longer corroboration of the same kind.

The other series of observations to which I must now call your attention are those of Mr. Henderson, made at the Cape of Good Hope, on the great star α Centauri, the third star in brightness which the heavens offer to our view*. It is a magnificent double-star con-

[* See Phil. Mag., S. 3., vol. xiv. p. 316.—Ed.]

sisting of two individuals, the one of a high and somewhat brownish orange, the other of a fine yellow colour, and each of which I consider fairly entitled to be classed in the first magnitude*. Their distance is at present about 15″ asunder, but it is rapidly diminishing, and in no great lapse of time they will probably occult one another, their angular motion being comparatively small. Their apparent distance was formerly much greater: how much we cannot say for want of observations, but probably the major axis of their mutual orbit is little short of a minute of space. They, therefore, afford strong indications of being very near our system. Add to which their proper motion is very considerable, and participated in by both, which proves their connexion as a binary system; and an additional presumption in favour of their proximity may be drawn from their situation in what, from general aspect, I gather to be the nearest region of the milky way, among an immensity of large stars.

Mr. Henderson observed these stars with great care both in right ascension and declination with the very fine transit, and (in spite of certain grievous defects in the axis) the otherwise really good and finely divided mural circle of the Royal Observatory in that colony. Since his return to England, he has reduced these observations with a view to parallax, and the result is the apparent existence of that element to what, after what has been said, we must now call the great and conspicuous amount of a full second. Mr. Main, to whom I am so largely indebted for allowing me to draw so freely on his labours, has also discussed these results, and comes to the conclusion that (as might, perhaps, be expected) the right-ascension observations afford a trace, but an equivocal one, of parallax, but that in declination (I use his words) "The law of parallax is followed remarkably well. There is scarcely an exception to the proper change of sign, according to the change of sign of the coefficients of parallax. This is quite as much as can reasonably be expected in a series of individual results obtained from any meridional instrument for observing zenith distances. We cannot expect to find the periodical function regularly exhibited by the differences. On the whole, therefore, we should say that, in addition to the claims of α Centauri on our attention with relation to its parallax, arising from its forming a binary system, its great proper motion, and its brightness,—it derives now much additional importance, in this point of view, from the investigation of Mr. Henderson. This we are at least entitled to assume until some distinct reason, independent of parallax, shall have been assigned for the changes in the declinations. Such I do not consider impossible, having before my eyes the results which Dr. Brinkley derived, in the cases of certain stars, from the Dublin circle. For the present it must be considered that the star well deserves a rigorous examination by all the methods which the author himself has so well

* I have seen *both* their images projected on a screen of three thicknesses of stout paper, the eye being on the opposite side of the screen from that on which the images were depicted.

pointed out; and that, in the event of a parallax at all comparable with that assigned by Mr. Henderson being found, he will deserve the merit of its first discovery, and the warmest thanks of astronomers, as an extender of the knowledge which we possess of our connexion with the sidereal system."

With this view of Mr. Henderson's labours I fully agree, and await with highly excited interest the result of Mr. Maclear's larger and complete series of observations on this star both with the old circle and with that more perfect one with which the munificence of government has recently supplied the observatory. Should a different eye and a different circle continue to give the same result, we must, of course, acquiesce in the conclusion; and the distinct and entire merit of the *first* discovery of the parallax of *a* fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in so far anticipating a decision which time alone can stamp with the seal of absolute authenticity.

Gentlemen of the Astronomical Society, I congratulate you and myself that we have lived to see the great and hitherto impassable barrier to our excursions into the sidereal universe; that barrier against which we have chafed so long and so vainly—(*æstuantes angusto limite mundi*)—almost simultaneously overleaped at three different points. It is the greatest and most glorious triumph which practical astronomy has ever witnessed. Perhaps I ought not to speak so strongly—perhaps I should hold some reserve in favour of the bare possibility that it may be all an illusion—and that further researches, as they have repeatedly before, so may now fail to substantiate this noble result. But I confess myself unequal to such prudence under such excitement. Let us rather accept the joyful omens of the time, and trust that, as the barrier has begun to yield, it will speedily be effectually prostrated. Such results are among the fairest flowers of civilization. They justify the vast expenditure of time and talent which have led up to them; they justify the language which men of science hold, or ought to hold, when they appeal to the governments of their respective countries for the liberal devotion of the national means in furtherance of the great objects they propose to accomplish. They enable them not only to hold out but to redeem their promises, when they profess themselves productive labourers in a higher and richer field than that of mere material and physical advantages. It is then when they become (if I may venture on such a figure without irreverence) the messengers from heaven to earth of such stupendous announcements as must strike every one who hears them with almost awful admiration, that they may claim to be listened to when they repeat in every variety of urgent instance, that these are not the last of such announcements which they shall have to communicate,—that there are yet behind, to search out and to declare, not only secrets of nature which shall increase the wealth or power of man, but TRUTHS which shall ennoble the age and the country in which they are divulged, and by dilating the intellect, react on the moral character of mankind. Such truths are things quite as worthy of struggles and sa-

crifices as many of the objects for which nations contend, and exhaust their physical and moral energies and resources. They are gems of real and durable glory in the diadems of princes, and conquests which, while they leave no tears behind them, continue for ever unalienable.

It must be needless for me to express a hope that these researches will be followed up. Already we have to congratulate astronomy on the resolution taken by one of our great academic institutions to furnish its observatory with an heliometer of the same description as Bessel's; nor can we fear but that the research will speedily be extended to other stars, offering varieties of magnitude and other indications to draw attention to them.

On the whole, then, the award of our medal, which the Council have agreed on, seems to me, under the circumstances, fully justified. I will now request the Foreign Secretary to convey it to our distinguished Associate; and in so doing, I will add our hope that, in the painful and distressing visitation with which it has pleased Providence recently to try him, he may find occasion to withdraw his mind awhile from that melancholy contemplation to receive with satisfaction such a tribute to this his last and perhaps his greatest achievement, accompanied as it is by the truest regard for his private worth and the most respectful sympathy for his present distress.

The Meeting then proceeded to the Election of the Council for the Year 1841, when the following Fellows were elected, viz.

President: the Hon. John Wrottesley, M.A.— *Vice-Presidents*: George Biddell Airy, Esq., M.A., F.R.S., *Astronomer Royal*; Sir John F. W. Herschel, Bart., K.H., M.A., F.R.S.; John Lee, Esq., LL.D., F.R.S.; Rev. Richard Sheepshanks, M.A., F.R.S.—*Treasurer*: George Bishop, Esq.—*Secretaries*: Rev. Robert Main, M.A.; Lieut. Henry Raper, R.N.—*Foreign Secretary*: Richard W. Rothman, Esq., M.A.—*Council*: Francis Baily, Esq., F.R.S.; Rev. W. Rutter Dawes; Augustus De Morgan, Esq.; George Dollond, Esq., F.R.S.; Bryan Donkin, Esq., F.R.S.; Rev. George Fisher, M.A., F.R.S.; Thomas Galloway, Esq., M.A., F.R.S.; Edward Riddle, Esq.; Captain W. H. Shirreff, R.N.; Lieut. William S. Stratford, R.N., F.R.S.

March 12.—The following communications were read:—

On a Reformation of the Constellations, and a Revision of the Nomenclature of the Stars. By Dr. Olbers. Translated and communicated by Sir J. F. W. Herschel, Bart.

From the earliest periods of history, it has been found that as soon as mankind turned their attention to astronomy, it became requisite to group the stars into different constellations, whose outlines might seem to designate various figures. Hence arose those several constellations that have been handed down to us from the Indians, Chinese, Egyptians, Persians, Arabians, Peruvians, and others; as well as those which the Greeks adopted, and which have survived to our own times. However curious and interesting

these constellations may seem, yet few of them will be found to express with any accuracy the figures which they are intended to represent. It is probable that out of the strange mixture of men, animals, and other objects, which the first astronomers invented, the imaginative Greeks made the present combination or selection. That the Greeks derived these constellations from an Asiatic people, and that they did not (as Newton supposed) invent them shortly after the Argonautic expedition, is evident from their not being able, for some time at least, to explain the constellations according to their mythology. I need only mention the Greek constellation Εν γονασιν, or kneeling figure, which we now call Hercules, and the Ορνις, or bird, which we now designate as the Swan; neither of which are explained in their mythology. Combined with this mythology the constellations were sung by the Greek and Roman poets, and are now become classical. In the present advanced state of astronomy, however, we do not arrange the stars wholly according to the constellations, but according to their right ascensions; yet these constellations are a valuable assistance towards an artificial memory, and afford us an excellent method whereby we are enabled to know and distinguish the various stars in the heavens, and to remember and record their places. The ancient Greeks reckoned only forty-six constellations, or, at most, forty-seven, if we include the Χηλαι, or claws, of the Scorpion as a separate constellation, which had been denoted as Libra before the existence of the Alexandrian school; to which Hipparchus added the forty-eighth, namely, Equuleus. The flattery of some courtiers was exerted to create two other constellations, viz. the Hair of Berenice and Antinous, but without success; till Tycho at last gave them a permanent place in the heavens.

In the fifteenth century, when navigation was extended beyond the equator, and sailors noted those stars in the southern hemisphere which were not visible to the ancients, they also found it convenient and useful to adopt the same plan of grouping the new stars into constellations. They did not, however, adapt them to the Greek mythology, but selected principally such objects as presented themselves in the newly-discovered countries: whence we have, for the southern constellations, the Phœnix, the Toucan, the Little Water-snake, the Sword-fish, the Flying-fish, the Fly, the Chameleon, the Bird of Paradise, the Peacock, the Indian, and the Crane. The ancients took only those parts of the heavens, as the ground-work of the constellations, where the bright stars existed: consequently, there were many places where there were no constellations, and the stars which were scattered over such situations were called *αμορφωτοι*, or *informes*. There was no inconvenience in this: but some of these empty spaces were very great, and exhibited here and there stars that seemed to be as much entitled to be formed into a constellation as several of the existing ones. Therefore modern astronomers, as Bartschius relates, (and, perhaps, he himself, partly) invented and formed the new constellations called the Camelopard, the Unicorn, the Fly, and the rivers Jordan,

Euphrates, and Tigris. The heavens would now appear to be sufficiently covered, and it seemed that all the advantages which the constellations could give to the memory and imagination, in learning astronomy, had been obtained. But when Hevelius, in the latter part of the seventeenth century, had finished, with incredible labour and care, his valuable catalogue of stars, he considered that those persons who were not observers themselves had no right to institute new constellations: and although, with much reluctance, he retained the Camelopard, the Unicorn, and the Fly of Bartschius, yet he rejected the rivers; and instead of them, and in some other vacant spots, introduced the Hounds, the mountain Menalus, Cerberus, the Fox and Goose, the Lizard, the Shield of Sobieski, the Lynx, the Little Lion, the Little Triangle, and the Sextant, and also gave to Antinous a bow and arrow. Unnecessary as this increase of the constellations may be, the indefatigable Hevelius may be allowed to retain it, as the best means of preserving a remembrance of his great work. For his enumeration and classification of the fixed stars, for which he had sacrificed the greatest part of his life, his strength, and his fortune, and by which he hoped to have gained immortal fame as an astronomer, was soon after doomed to yield to the better and more complete British catalogue of Flamsteed, and has now become nearly useless. Astronomers now no longer use it: nor, indeed, can they use it, except in a few cases, and in some researches of little importance. Hevelius's constellations may be said to be analogous to the ancient ones; some of them may be considered as mythological: and as to the rest, they, for the most part, represent animals. Therefore it may be stated, if not in recommendation, at least in defence of them, that if they overwhelm our maps of stars they do not disfigure them. We have got on our maps only two of the constellations that were introduced in the seventeenth century, namely, Charles's Oak and the Brandenburgh Sceptre; for the Heart of Charles is merely the name of a star, and no constellation. Halley had formed Charles's Oak out of the stars that belonged to Argo; and, notwithstanding the protest of Lacaille against this usurpation, this constellation still remains. Kirch was desirous of introducing the Swords, the Orb, and the Sceptre of Brandenburgh. The electoral Swords were covered by the mountain Menalus; and the Orb yields its place to the Bow and Arrow which Antinous had received from Hevelius; and although the new globes very often disarmed Antinous, yet he has not yet taken the Orb in his hand. Moreover the Sceptre of Brandenburgh, although it did not interfere with any other constellation, yet would not have had a place on our globes, if Bode had not been the astronomer-royal of Prussia. The Cock, which was formed from a portion of the ship Argo, has likewise disappeared; the Sceptre of Louis XIV. with which Royer wished to honour his sovereign, yielded its place to the Lizard of Hevelius; the French Lily could not push away the Fly; and so on with many others; for example, the Little Crab, the South Arrow, &c. are quite forgotten, and not even known at the present day. One would now suppose that nearly

eighty constellations were quite enough for all useful purposes: but the vanity of introducing new constellations had, in the eighteenth century, exceeded all bounds, and twenty-six more were added to the number. This extravagant number of new constellations, some of which were formed of scarcely visible stars, by no means made the study of astronomy more easy; but, on the contrary, confused it, and rendered it more difficult. Moreover these new constellations are so unsuited to the others, and chosen with so little taste, that no one can look on our modern globes without disgust. The first who introduced this objectionable system was the excellent and distinguished Lacaille. Surely if it were requisite that the whole heavens should be filled with constellations, they might have been chosen according to some general principle. We might have embellished the apparatus and inventions of our chemists, if indeed they could be embellished by them: and as the ancient figures of heroes and animals must be retained, some latitude might be allowed also to astronomical instruments. But figures, like the shop of the sculptor, the chemical furnace, the easel, the microscope, the air-pump, &c., have no relation to the sky, and their being mixed up with the others is heterogeneous, disagreeable, and without any taste. The same remark will apply to the Printing-press and Electrical machine of Bode; also to Lalande's Air-balloon, although this latter constellation may seem to have some connexion with the heavens. The Hermit-bird (*Solitarius*) of Le Monnier might remain, if it did not interfere with Libra; but his Reindeer is quite absurd, on account of its smallness, it being scarcely so large as the Lizard, and much smaller than the Hare. Also Lalande's Messier, which is covered by the horns of the Reindeer, is but a very small figure in comparison with the immense human figures of the ancients that surround him, although it has robbed the greater part of its little possession from Cepheus and Cassiopeia, and has contracted the throne of the latter into a bent form; the little man is in fact quite ridiculous amongst such enormous figures. The immortal name of Frederick the Great needed not the aid of a constellation for its preservation; a constellation, in fact, carved out of Andromeda, and styled *Honores Frederici.* And if it be an honour to this great monarch to have his name enrolled amongst the stars, we must bear in mind that he enjoys this apotheosis, not only with the brave Sobieski, but also with Poniatowski and the insignificant Charles II. of Great Britain. The name of George III. will also be handed down to posterity without the George's Harp of Pater Hell: and the discovery of Uranus will preserve the name of Herschel as long as astronomy exists, without the necessity of placing his telescope in a narrow slip in the heavens. By the Lion and the Lynx the feline tribe had been sufficiently represented in the sky, without any necessity of introducing a Cat amongst the stars, merely because Lalande was fond of this domestic animal. I appeal to the judgement of all those who have compared any of the old celestial maps with the more modern ones, whether they do not feel a repugnance to the absurd mixing of so many heterogeneous constellations. And since by such

an immoderate number of them the knowledge of the stars is rendered more difficult, and the taste vitiated, I would entreat astronomers to assist in freeing the heavens from such an useless accumulation, and to remove all the constellations that have been introduced since the time of Hevelius and Flamsteed. If it should be found desirable to take away some of Hevelius's constellations, and even to retain some of those which have been introduced in the eighteenth century, there should be no partiality shown, so as to endanger the wished-for uniformity in our maps; therefore, it might appear quite unnecessary for me to fix a precise point where the line was to be drawn. It would be advisable also that the constellations should be delineated in such an uniform manner in all maps that there should be the same stars in the same parts of the figures. It is true that we do not, after the manner of the ancients, and of Hevelius, denote different stars merely by their place, but more distinctly by letters or numbers; yet it is very useful if we could at once denote the place which a new phænomenon (for example, a comet) has taken, and also the direction of its motion, by the portions of those constellations in which it was observed. We might, in this regard, take the figures in Flamsteed's great atlas as our types; and with the more propriety, since Flamsteed has constructed them according to the ancient figures and the descriptions given by Ptolemy; with this exception, that some of his figures are ugly and badly drawn. This is a point, however, that might easily be remedied, by following the beautiful and pleasing forms of Senex, Vaugondy, Pater Chrysologue, and others. But when once the proposed forms have been adopted there should be no further uncertainty or deviation *.

Continuation of the Investigation for the correction of the Elements of the Orbit of Venus. By Mr. Glashier.

In this paper the author has combined the equations formed for the correction of the elements from the Greenwich observations for the year 1839, with those given in his preceding paper, a notice of which is contained in the eighth Number of vol. v. of the Monthly Notices [or Phil. Mag., Third Series, vol. xviii. p. 601.].

Venus was near her inferior conjunction in the autumn of the year 1839, and the observed errors were consequently very well suited for the correction of the elements. The results deduced now depend on eighty-two equations, which, the author remarks, are formed from the combination of as great a number of observations as have ever been applied in determining the elements of any planet.

The formation of the equations is arrived at in precisely the same manner as in the preceding paper, and the resulting corrections to the elements are as follows:—

Correction of the Semi Axis Major.... $= -$ 0·00000776
—— Eccentricity........ $= +$ 0·00002303

[* On the subject of this memoir, see an abstract of a paper by Sir John Herschel, in the Proceedings of the Society for June, *suprà*, p. 582.—Ed.]

Correction of the Epoch of Mean Long.	= +	$2''{\cdot}13$
—— Aphelion..........	= +	225
—— Inclination	= +	$3{\cdot}23$
—— Long. of the Node ..	= −	$21{\cdot}40$.

And Lindenau's elements corrected, for the epoch Jan. 1, 1836, are—

Epoch of Mean Long.	=	$11^s\ 2^\circ\ 1'\ 35''{\cdot}23$
—— Aphelion..	=	10 9 15 3
—— Node	=	2 15 12 3 ·60
—— Eccentricity	=	0·00684568
—— Inclination	=	$3^\circ 23'\ 34''{\cdot}33$.

April 7.—The following communications were read :—

Occultations of Stars by the Moon, observed at Ashurst and Dulwich, in the year 1840. By Robert Snow, Esq.

Observations on the Variability of the Star α Cassiopeiæ during the years 1839 and 1840. By Robert Snow, Esq.

Description of the Observatory erected at Starfield, by W. Lassell, Esq.

The author states that this observatory was erected in the summer of the year 1839 at Starfield, near Liverpool, in latitude 53° 25′ 7″ north, and longitude $0^h\ 11^m\ 41^s$ west of Greenwich. The account of the building and instruments is accompanied by drawings and a model, giving a very distinct idea of the ground-plan and elevation of the building, and of the relative position of the instruments. The building consists of one circular apartment, of 14 feet 6 inches diameter, surmounted by a dome, and is based upon a structure of undisturbed clay. The two instruments contained within it are a transit instrument and an equatoreal instrument, both supported on parts of the same pier, which is 11 feet in length, the part supporting the transit instrument being 3 feet 6 inches broad, and that supporting the equatoreal, 2 feet 8 inches broad; and the whole of the pier is kept quite separate from the walls, to avoid any communication of tremor. The dome revolves on eight rollers of 6 inches in diameter, placed at equal distances, and the friction is so small, that the strength of a boy of twelve years of age is quite adequate to turn it. The transit instrument is placed a little to the west of the meridian line, passing through the centre of the dome, and by revolution of the dome commands the whole 180° of sky.

The telescope of the equatoreal is a Newtonian reflector of 9 inches clear aperture, and of 112 inches focal length. The object-mirror the author believes to be of very great perfection of figure, showing the stars in the most favourable states of the atmosphere with perfect roundness, and answering very well to the severe test recommended by Mudge in the Phil. Trans., of alternately exposing different portions of its surface to the rays from the object. The penumbra of a star seen out of focus is truly round, and equal in brightness in its various parts, presenting also the same form on both sides of the place of distinct vision.

The mirror is mounted in a copper tube, 9 feet long and of 9½ inches in diameter, and there is a peculiar contrivance, by means

of a lever and a weight, which are brought into action for great altitudes, but which have no effect when the telescope is horizontal, for preventing its flexure, and for securing its steady adjustment in all positions. The effect of this the author describes as rendering the images of stars equally round at all altitudes, and certainly never better than when near the zenith. The declination and hour-circles are each divided to 15′, and by means of verniers can be read to 30″ and 2^s respectively. The polar axis is made in three parts. The lowest part is a hollow cone of metal half an inch thick, the lower end being solid, and turned into a ball 2 inches in diameter. The ball is received into a nearly hemispherical brass step, provided with adjusting-screws. The upper part of the cone is turned and driven into the bored, internal surface of a cap-plate or flange, which affords an attachment almost as firm as if the whole had been cast in one piece. The exterior circumference of the collar of the flange is also turned with great care, and rests on two cast-iron friction-wheels, 7 inches in diameter, with axles of steel. The only bearings of the motion in right ascension being the step at the foot of the axis, and two points in the circumferences of the friction-wheels, the motion is rendered very smooth, and the required impulse for motion as small as can be desired.

For observation of objects at great altitudes, a very simple contrivance is made to elevate the observer conveniently to the required position at the eye-end of the telescope. Fastened to the following side of the opening of the dome is a stiff bar of iron, notched in the manner of a saw, upon which is hung, by iron hooks, a wooden frame, in which a moveable foot-board is adjustable to different heights, in the manner of the shelves of a bookcase, by which combination of elevations every variety of altitude can be obtained, at the same time that the sides of the frame afford an agreeable support to the observer. The operation of advancing the opening of the dome, also carries on the observing-ladder, and the position of the notched bar is such that the sliding of the ladder upon it, for the most part gives the necessary variation in altitude, and is very appropriately fitted to the change of position of the eye-tube of the telescope. The object-end of the telescope and the declination-circle are counterpoised by weights applied to the framework, not to the telescope itself, by which it remains in equilibrio in all positions. With a view of conveying as correct an impression as possible of the general arrangement of the parts of the observatory and the instruments, the author has accompanied his description by a model, on a scale of one-tenth the full size in linear dimensions.

Observations on Bremicker's Comet, with its apparent places, as obtained, with the Equatoreal, at Mr. Bishop's Observatory, and an account of the methods employed in deducing them. By the Rev. W. R. Dawes.

The extreme faintness of this comet rendering it observable only by persons possessing large telescopes, Mr. Dawes was induced to employ for the purpose Mr. Bishop's equatoreally-mounted telescope of 7 inches clear aperture, and $10\frac{3}{4}$ feet focal length. The micro-

meter used has four thick wires, two of which are parallel to the screw, and therefore at right angles to the webs, and fixed at an angular distance of 7″ between their adjacent edges, and the other two parallel to the webs, and moveable with them. By means of these four thick wires a small square was formed, as nearly as possible in the centre of the field, and the nucleus of the comet placed in it for observation.

The nucleus resembled a star of about the 10th or 11th magnitude, abruptly diffusing itself into the nebulosity around it. It was very decided, with powers 63 and 105, the latter of which was generally employed in the observations. The first day of observation of the comet was Nov. 14, when, as well as on Nov. 16, 19, and 24, the stellar nucleus was remarked as distinctly visible. On Nov. 19 it appeared rather eccentric on the north following side. After Dec. 3 a succession of cloudy evenings occurred, and the comet was not seen again till Dec. 22, when the stellar nucleus was again visible, and the apparent diameter of the comet was certainly larger. It must also have been brighter, since the observations of it were considered good, though the sky was so hazy that stars of the 4th magnitude were scarcely visible to the naked eye.

The last night of observation was Dec. 29, when it still exhibited a decided stellar nucleus, and the nebulosity appeared more extended and dense. No observations were made to determine its diameter, but it was estimated at $1\frac{1}{4}'$. At no time has any appearance of a tail been suspected.

The method of observation adopted by Mr. Dawes was that of transit-comparisons in right ascension, and of micrometrical comparisons in north polar distance with neighbouring stars whenever it was practicable. Most of the comparison-stars have been identified as contained in the Catalogue of Groombridge; and the author enters greatly into detail respecting the catalogue-places of all his observed stars, preferring the places given in Pond's Catalogue of 1112 stars, whenever they were found there.

A table annexed to the paper, and given in No. 14 of the Monthly Notices, exhibits the observed apparent places of the comet.

May 14.—The following communications were read:—

Description of a Dioptric Telescope, and of a Micrometrical Lunette. By M. Chevalier. Translated by R. W. Rothman, Esq.

The dioptric telescope constructed and described by M. Chevalier differs from other telescopes chiefly in the arrangement of the object-glass. This consists of a large achromatic combination of long focus, and of a small one which reunites the rays transmitted by the primary one. The chief advantage derived from this construction is the great diminution of spherical aberration, and, consequently, the power of increasing the aperture of the telescope with the same focal distance. This advantage is obtained by dividing the excess of curvature between the two achromatic combinations, and by calculating the distance between them, so as best to secure the diminution of the spherical aberration and perfection of the achromatism. The author considers that a great advantage is gained by the process

which he adopts of *luting* together the crown and flint-glasses forming the achromatic combinations, by means of balsam of Canada almost cold, this process being not subject to the inconvenience attached to the use of mastic in drops recommended by other artists. He also suppresses the use of diaphragms in the tubes of his telescopes, which he lines with black velvet.

The author's micrometrical lunette consists of a telescope bent at right angles, in the manner of a Newtonian reflector, carrying a sight on the object-end and a small perforated mirror on the eyepiece, which combination permits the observer to see at once, and *with the same eye*, an object at a fixed distance, and the image of the object produced by the telescope. All the usual micrometrical and illuminating methods are applicable to the sight, on which may be traced divisions as small as we choose, being read by a small telescope parallel to the first, and placed before the metallic mirror. By the author's arrangement, the errors to which common micrometers are subject, depending on the thickness of the wires, the imperfection of the screws, &c. are got rid of. He considers his micrometer as particularly applicable to geodetic operations, and suitable for the purposes of the surveyor as well as of the astronomer.

Observations of the Aurora Borealis. By Robert Snow, Esq.

In this paper Mr. Snow records his observations of this interesting phænomenon made at Ashurst and Dulwich, from the autumn of the year 1834 to the autumn of 1839, within which period several remarkable auroræ appeared; among which, as particularly deserving notice, he describes the various appearances of those of November 17, 1835, and of February 18, 1837; the latter being rendered additionally remarkable by its happening on the same evening as the occultation of Mars by the moon. Other fine auroræ described by the author appeared on November 12, 1837; on September 13, 1838; and on January 19 and September 3, 1839.

The author deduces from his observations the following invariable circumstances of the phænomenon:—That the aurora may be expected at any season of the year; that it assumes nearly every variety of colour; that it resembles both in shape and motion every variety of ordinary cloud; that its appearances are, in the course of the same evening and without any determinate order, undulating, radiating, and streaming, with other capricious forms not easily expressible; that the length of time during which it is visible is very uncertain; that it appears to the eye (geometrical considerations apart) as if it existed at various distances from the earth's surface; that, although for the most part it is not influenced by the presence of clouds, it occasionally tinges them with its own prevailing colours; that this has been noticed only when the clouds are low; that there are also certain *lofty cirrous* clouds, which have the appearance of arranging themselves in peculiar bands or strata, as if in connexion with the aurora; that these strata are visible during daylight, when the visibility of the *dark portion of the arch* has sometimes been strongly suspected; that the stars are seen both well and ill defined through the auroral light and the auroral darkness; that it is by no

means confined to the northern regions of the sky, though originating about the magnetic north; that, with the exception of a diminution of its general effect, it is uninfluenced by moonlight; that its appearance generally accompanies weather the reverse of frost, such as heavy wind and rain; and, lastly, that it is wholly inaudible. The author concludes by warning the spectators of this phænomenon against the false impressions to which the senses are liable, especially with regard to the sensation of heat and the notion of sound, as attending phænomena in which our idea of either of these qualities has been predominantly awakened.

Mean Positions of the Stars mentioned in Mr. Baily's Address to Observers, determined at San Fernando in the years 1834, 1835, 1836, 1837, and 1838. By M. Montojo. Translated by Captain Shirreff, R.N.

The whole number of stars contained in this catalogue is 126, of which 121 are mentioned in Mr. Baily's "Address to Observers," as being of doubtful position, or not recently determined. The reductions of the observations of right ascension have been performed strictly by means of the *Tabulæ Regiomontanæ*, the mean positions of the fundamental stars and the numbers and constants of that work having been adopted. The equinox to which they are referred is that of Bessel, coinciding with that determined at San Fernando from 206 observations of the sun.

The transit-instrument and clock employed are described in the published Observations of the years 1834 and 1835; and the instrumental errors there explained have been rigorously taken into account.

The observations in declination include only the years 1837 and 1838, the mural circle having come into operation in 1837.

This instrument differs in no essential respect from those of the Observatory of Greenwich, and was made by Mr. T. Jones.

The mode of observation is essentially similar to that pursued with other mural circles, the horizontal points being obtained by direct and reflexion observations, at the same transit, of different stars ranging through a large arc of zenith distance, and the mean of the results of three days' observations being usually employed. Each observer is made to determine his own horizontal point, some slight disagreement between the results of different observers, similar to what in transit observing is called *personal equation*, having been recognised.

The latitude was determined by superior and inferior transits of Polaris, of β, δ, and λ Ursæ Minoris, and of Cephei 51 (Hev.), using only such observations of them as were made by direct vision and by reflexion at the same transit.

The refraction is computed from the *Tabulæ Regiomontanæ*, and the precession from the values of m and n given in that work.

The resulting right ascensions are compared with the Astronomical Society's Catalogue, with Pond's Catalogue of 1112 Stars, with Wrottesley's Catalogue, and with Johnson's of 606 Southern Stars, and also with the stars found in the Greenwich observations of 1833, 1836, and 1837.

The N. P. D.'s are compared with the Astronomical Society's Catalogue, with Pond's, Johnson's, and Henderson's Catalogues, and with the Greenwich observations of 1836 and 1837.

June 11.—The following communications were read:—

A New Catalogue of Moon-culminating Stars, observed at South Kilworth. By the Rev. Dr. Pearson.

This catalogue contains 520 stars which fall within the passage of the moon, in every situation of the nodes, and which had been published in the Appendix to the first volume of the author's work on Practical Astronomy *. The stars in this catalogue had been selected by Caturegli from the determinations of different observers, and therefore might be supposed to have their places assigned with different degrees of accuracy, so as to require a new series of careful observations, and reduction with improved constants, to render them suitable objects for so delicate a purpose as the determination of longitudes, by comparison with the moon's limb, upon or near the meridian. This consideration induced Dr. Pearson to confine his stellar observations to moon-culminating stars, employing for the purpose, contemporaneously, the transit instrument and circle, described in his work before referred to, and a clock by Hardy, which is an exact fellow to the transit-clock at Greenwich in its original state. The observations were continued uninterruptedly from November 4, 1830, to May 7, 1834; when, on account of inconvenience arising from the smoke of the village, the instruments were removed to another building in the author's private grounds, lying in the same meridian and 4″ to the south of the former. They were here continued till the end of the year 1837, when it was found that, with very few exceptions, the whole of the stars had been observed at least six times.

The reductions were made with the constants given in the Astronomical Society's Catalogue for the epoch 1830, for the purpose of direct comparison with that catalogue, using the annual precessions given in that work. Brinkley's refractions were used in the reduction of the circle observations, which include stars as low as 8° of altitude, and may therefore possibly furnish data for correction of the tables of refractions; with which view the original observations have been preserved. The telescopes used are by Tulley, and the object-glasses, of 3¼ inches aperture and nearly 44 inches focal length, may be considered excellent in their definition of a large star. The collimation in altitude was determined by a small collimating telescope, by reversion of the azimuthal positions; and comparison of the sum of reversed readings with 90° proved highly satisfactory in every instance.

The resulting right ascensions of the stars used for clock-error are given in an appendix, that their close agreement with the right ascensions derived from a multiplicity of observations at Greenwich may be exhibited.

Numerical corrections were not applied for the instrumental errors, but the instruments themselves were kept in adjustment by

[* See Phil. Mag., Second Series, vol. iv. p. 134.—Ed.]

means of hanging levels. The meridian mark on the northern wall of a house, directly to the south of the transit instrument, was never observed to deviate in position; though the pillar, carrying the northern mark, was liable to deviations when the sun was powerful or the frost severe.

On the Advantages to be attained by a Revision and Rearrangement of the Constellations, with especial reference to those of the Southern Hemisphere, and on the Principles upon which such Rearrangement ought to be constructed. By Sir J. F. W. Herschel, Bart., K.H., V.P.R.A.S., &c. &c.

The stars must have been divided into groups, which, as also single stars, would have received names long before they began to be considered with reference to the seasons, or the sun's motion in the ecliptic. The best-defined groups would be the first named, but in the stars of the zodiac this principle would be afterwards modified, for the convenience of dividing the circle into equal parts. In the extra-zodiacal regions no principle seems to have prevailed, either of subdivision or nomenclature; and many of the figures of the constellations scarcely resemble the forms intended, with the exception of a few, as Scorpio for example. Names, however, would be imposed from other associations than mere form. The author proposes to consider how far the present system of constellations is adapted to the purposes of astronomy, and to examine by what modifications it may be made more serviceable; and also to inquire into the circumstances which render a systematic revision of them desirable, or even necessary, at the present advanced epoch.

The use of constellations to the astronomer is to enable him to refer to a particular star. For this purpose, a distribution on any principle would serve; yet even on this point the present system often leads to confusion: for, 1st, the similarity of the names of several constellations, and the bestowing on a new constellation the name of an old one, with an adjunct for the sake of distinction, render it necessary in many cases, in order to avoid confusion, to write the names at full length: thus, for example, we have (with several others) Ursa major and minor, three Triangles, Pisces and Piscis, Apis and Apus, Telescopium repeated three times, Quadrant, Sextant, and Octant, &c. &c. 2ndly, Some constellations are so extensive that they exhaust three or more alphabets, and therefore it is sometimes necessary, beside the letter of the star, to specify also its right ascension and declination: for example, in Argo three stars are marked *a*, and seven A. Again, such constellations extend over so many hours of right ascension, that the name of the constellation is of no use in finding, in the catalogues, one of the stars composing it: thus, Argo occupies eight hours of right ascension. 3dly, The imperfect and uncertain boundaries of the present constellations lead to confounding the Greek letters of one constellation with those of another contiguous to it. Moreover, the boundaries are not always the same in different maps—a circumstance which alone is decisive of the necessity of some system which might be more favourable to a general understanding.

To the astronomer who refers to stars by their catalogued places, these inconveniences may not afford serious ground of complaint; since even in the southern hemisphere but few stars of the fifth magnitude remain uncatalogued. Nor are the defects of the present system felt by seamen, who have little to do with the constellations beyond referring to a few well-known stars. But to those who employ themselves in the physical departments of practical astronomy, such as variable stars, photometrical determinations, and other subjects, and who require a perfect familiarity with the aspect of the heavens, the present arbitrary and capricious allotment of the stars renders reference to maps constantly necessary. And when the leading stars in the map are not those which in the heavens catch the eye by their brightness, it becomes necessary alternately to inspect the map by candlelight, and then to rush out into the darkness, to compare the impression made on the memory with the visible aspect of the stars, to the loss of all delicacy of vision, and the injury of the organ itself.

The author had proposed to himself to follow out, in the southern hemisphere, the plan adopted by the late Sir William Herschel, in order to place on record the relative apparent magnitudes of the stars at this epoch; and it was while thus engaged that he became impressed with the necessity of a total reformation of the ancient system.

In enumerating the qualities which a system of sidereal arrangement should possess, the writer observes that the new subdivisions should be of moderate extent, the figures easily traced, and the groups such as naturally arrange themselves in distinct forms. Again, the boundaries ought to be definite, so as to be transferred from one map to another without variation; they should accordingly be arcs of great circles, or parallels to a great circle; that is, circles of right ascension and parallels of declination. The limits, thus assumed, should correspond to a particular epoch, being reduced to any other time, by the necessary tables, like stars. The advantages of this system would be that each star would be at once referred to its proper district; that the observer, becoming familiar with the limits, would know the time when each star approached his meridian, as also the limits of altitude between which it would be comprised.

After some remarks on the mode of carrying the proposed plan into execution, the writer suggests:—

1. That the names of constellations should be such as not to be easily confounded either when spoken or printed.

2. Each name should be Latin, with a regular genitive case; to which the astronomers of different nations should conform, without translating them into their own languages.

3. In the names, low or homely associations, technicalities of science, and national and political allusions, should be avoided.

4. General names should be preferred, as Rex, Regina, Miles, Sculptor, Poeta, &c.; and they would be selected from mythology and classical antiquity, as neutral ground.

5. The naming of constellations after their imagined figures must be abandoned.

Individual stars are most conveniently designated by Greek letters, the letters of the alphabet being assigned in the order of the brightness of the stars. The inconvenience which would sometimes attend this last condition might be remedied.

The practice of giving a proper name to each star is so convenient, that the author would wish to see it extended to all stars of the third magnitude, at least; and he concludes with some further suggestions on this point. The paper is accompanied by a plate in illustration of the principle of arrangement*.

ROYAL IRISH ACADEMY.

Nov. 9. 1840.—The Rev. T. R. Robinson, D.D., M.R.I.A., gave the Academy an account of a large reflecting telescope, lately constructed by Lord Oxmantown, and of the processes employed in forming its specula†.

After explaining the relative importance of magnifying and illuminating power, Dr. R. proceeded to give a brief sketch of the history of the reflecting telescope, which seemed to have been forgotten for many years after its invention, till it was revived by Hadley. The labours of Short soon gave it celebrity; yet even this artist limited himself in almost every instance to sizes which were not more powerful than the achromatics of his day, and his large instruments appear to have been failures‡. It was not till a full century after the publication of Newton's paper, that Sir William Herschel gave this telescope the gigantic development which has crowned him with imperishable fame; and by the construction of telescopes of nineteen and forty-eight inches aperture, placed regions almost beyond the scope of measurement within the reach of human intellect. But as Short, in a spirit unworthy of his talents, took care that his knowledge should die with himself, and Herschel published nothing of the means to which his success was owing, the construction of a large reflector is still as much as ever a perilous adventure, in which each individual must grope his way. Accordingly, the London opticians themselves do not like to attempt a mirror even of nine inches diameter, and demand a price for it which shows the uncertainty and difficulty of its execution. In Ireland we are more fortunate, for a member of our Academy, Mr. Grubb, finds no difficulty in making them of ad-

[* On the subject of this paper see an abstract of a memoir by the late Dr. Olbers, in the proceedings of the Society for March, *autè*, p. 571. —Ed.]

[† An abstract of Lord Oxmantown's paper on his Telescope, read before the Royal Society, and published in the Philosophical Transactions, will be found in Phil. Mag., Third Series, vol. xvii. p. 380.—Ed.]

‡ A Newtonian of six feet focus, and 9·4 inches aperture, is said by Maskelyne to have shown the first satellite of Jupiter 13″ longer than a *triple* achromatic of 3·6 inches aperture. The telescope of twelve feet focus, and eighteen inches aperture, now at Oxford, showed multiple rings of Saturn.

mirable quality up to this size, or even fifteen inches; but with all his distinguished mechanical talent, he is believed to be doubtful of the possibility of more than doubling this last magnitude in perfect speculum metal.

Under these circumstances, too much praise cannot be given to Lord Oxmantown, who, in the midst of other pursuits, has found leisure for such researches; and by a rare combination of optical science, chemical knowledge, and practical mechanics, has given us the power of overcoming the difficulties which arrested our predecessors, and of carrying to an extent which even Herschel himself did not venture to contemplate, the illuminating power of this telescope, along with a sharpness of definition scarcely inferior to that of the achromatic.

The chief difficulties which are to be overcome in the construction of reflectors, arise from the excessive brittleness of the composition of which specula are made, and from the necessity of giving them figures which shall be free from aberration. The great mirror in the Newtonian form is (if the eyepiece and plane mirror be correct) the conical paraboloid.

It is necessary that speculum metal should possess, in the highest attainable degree, the qualities of whiteness, brilliancy, and resistance to tarnish. Lord Oxmantown has found that these conditions are best satisfied in the *definite* combinations of four equivalents of copper to one of tin; or by weight, 32 and 14·7 nearly. Metals differing from this by a slight excess of either component, are, when first polished, scarcely less brilliant, but are dimmed so rapidly that the lapse of a few days produces a sensible difference. On the other hand, some large specula of the atomic compound have been lying uncovered for years, without material injury to their polish.

But this compound is brittle almost beyond belief; a slight blow, or even the application of partial warmth, will shiver a large mass of it; though harder than steel, its surface is broken up with the utmost facility, and it has a most energetic tendency to crystallize. The common process of the founder fails with it, except for masses of very limited magnitude, as the cast cracks in the mould; and the subsequent difficulties of the annealing are such, that it has been a very general practice to use an alloy lower (containing more copper) than the atomic standard. Even Sir William Herschel was obliged to yield to this necessity. It appears from a letter of Smeaton, (Rees's Cyclopædia, Art. Telescope,) that for his 20-foot mirror of 19 inches aperture, the composition was 32 copper to 12·4 tin, and that for the 40-foot it was even lower; yet two out of three attempts to cast this huge speculum failed.

Lord Oxmantown at first endeavoured to evade the difficulty, by constructing a speculum in pieces, soldering plates of fine metal to a back of a peculiar brass, ascertained to have the same expansion; and has completed one of thirty-six inches aperture and twenty-seven feet focal length, which performs very well on stars below the fifth magnitude, but above that exhibits a cross formed by the diffraction at the joints; and in unsteady states of the air exhibits the sixteen

divisions of the great mirror on the star's disc. By diminishing the number and size of the joints it is found that these inconveniences can be diminished, so as to be scarcely perceptible; and in all probability this is the process by which the remotest limits of telescopic vision will ultimately be attained. It is, however, not necessary for instruments of even greater dimensions than this, since Lord Oxmantown has succeeded, by a contrivance as simple as ingenious, in casting at the first attempt a *solid* mirror of the same size; and there is no reason to suppose that it will be less effective on a much larger scale.

But however difficult it may be to obtain the rough speculum of large dimensions, it is still more so to give it a proper figure, combined with that brilliant polish which is technically called black, because it reflects no light out of the plane of incidence. In such mirrors as can be wrought by hand, they are worked by short cross strokes on the polisher, and at the same time have a slow rotation relative to it. This might be expected to produce merely a spherical figure; but by varying the length of the stroke, by circular movement, elliptic figure of the polisher, or removing portions of its pitch covering, a parabolic figure is obtained. For sizes above nine inches diameter, the work must be performed by machinery: but in all which Dr. Robinson has seen (the most remarkable of which are those of Sir William Herschel* and Mr. Grubb), the cross stroke is given by a lever moved by hand: and it is supposed that perfect results cannot be obtained but by the *feeling* of the polisher's action. Sir John Herschel is believed to have made important additions to his father's apparatus; and it is to be hoped he will soon redeem his promise (Mem. R. Ast. Soc., vol. vi.) of publishing his improvements.

Lord Oxmantown has in many respects deviated from the usual process. His polisher, of the mirror's diameter, intersected by transverse and circular grooves into portions not exceeding half an inch of surface, is coated, first, with a thin layer of the common optical pitch, and then with a much harder compound. It is worked *on* the mirror, and counterpoised, so that but little of its weight bears; but the want of pressure is compensated by a long and rapid stroke. The mirror revolves slowly in a cistern of water, maintained at a uniform temperature, to prevent the extrication of heat by friction. The polisher moves slowly in the same direction, while it is also impelled with two rectangular movements. The machine is driven by steam, and requires no superintendence, except to supply occasionally a little water to the polisher, and to watch when the polish is complete. By an induction from experiments on mirrors from six to thirty-six inches aperture, it was found that if the magnitudes of the transverse movements be $\frac{1}{3}$rd and $\frac{9}{100}$th of the aperture, and their times be to its period of rotation as 1 and 1·8 to 37, the figure will be parabolic: but to combine with this the highest degree of lustre, it is found necessary to apply, towards the close, a solution of soap in liquid ammonia, which seems to exert a specific action.

* Dr. Robinson had the good fortune to see this at Slough, in 1830, while at work on a twenty-foot mirror.

The certainty of the process is such, that the solid mirror of thirty-six inches aperture, after being scratched all over its surface with coarse putty, was, in Dr. Robinson's presence, perfectly polished in about six hours, and was placed in its tube for examination without any previous trial as to quality.

Lord Oxmantown has preferred the Newtonian to the Herschelian form, and, in Dr. Robinson's opinion, with good reason. In the latter, the inclination of the great mirror to the incident rays must deform the image*, and it is now known, that even with faint objects sharp definition is of high importance. It should, in fact, be a segment of a paraboloid, exterior to the axis; and though a theorem of Sir William Hamilton (Trans. Roy. Irish Acad., vol. xv. p. 97) might seem to indicate mechanical means of approximating to the figure, yet Dr. Robinson fears there would be greater difficulty in applying them than in enlarging the aperture of the Newtonian, so as to make up for the loss of light. Another serious objection is, that in the Herschelian the observer's position at the mouth of the tube must cause currents of heated air, which will materially interfere with sharpness of definition.

As to the loss of light by the second reflexion, Dr. Robinson thinks it has been much overrated, and expresses a wish that a careful set of experiments were made on reflexion by plane specula at various incidences, on prisms of total reflexion, and the achromatic prism, proposed as a substitute by Sir David Brewster.

As to the rest of the instrument, it may suffice to say, that it bears a general resemblance to that of Ramage, but that the tube, gallery, and vertical axis of the stand are counterpoised, so that one man can easily work it, notwithstanding its enormous bulk. The specula, when not in use, are preserved from moisture or acid vapours, by connecting their boxes with chambers containing quicklime, which is occasionally renewed. This arrangement (which also occurred to Dr. Robinson, and has been for several years applied by him to the Armagh reflector,) appears to be very effective in preserving the polish.

In trying the performance of the telescope, Dr. Robinson had the advantage of the assistance of one of the most celebrated of British astronomers, Sir James South; but they were unfortunate in respect to weather, as the air was unsteady in almost every instance; the moonlight was also powerful on most of the nights when they were using it. After midnight, too (when large reflectors act best), the sky, in general, became overcast. The time was from October 29th to November 8th.

Both specula, the divided and the solid, seem exactly parabolic, there being no sensible difference in the focal adjustment of the eyepiece with the whole aperture of thirty-six inches, or one of twelve; in the former case there is more flutter, but apparently no difference in definition, and the eyepiece comes to its place of adjustment very sharply.

* Any one who has a Newtonian telescope can verify this, by inclining a little the great mirror, so however as not to pass the edge of the plane mirror by the pencil. In Lord Oxmantown's instrument, an inclination of 11′ sensibly injures it; were it Herschelian, the inclination must be 3° 11′.

The solid speculum showed α Lyræ round and well defined, with powers up to 1000 inclusive, and at moments even with 1600; but the air was not fit for so high a power on any telescope. Rigel, two hours from the meridian, with 600, was round, the field quite dark, the companion separated by more than a diameter of the star from its light, and so brilliant that it would certainly be visible long before sunset. ζ Orionis, well defined, with all the powers from 200 to 1000, with the latter a wide black separation between the stars; 32 Orionis and 31 Canis minoris were also well separated.

It is scarcely possible to preserve the necessary sobriety of language, in speaking of the moon's appearance with this instrument, which discovers a multitude of new objects at every point of its surface. Among these may be named a mountainous tract near Ptolemy, every ridge of which is dotted with extremely minute craters, and two black parallel stripes in the bottom of Aristarchus.

The Georgian was the only planet visible; its disc did not show any trace of a ring. As to its satellites, it is difficult to pronounce whether the luminous points seen near it are satellites or stars without micrometer measures. On October 29th, three such points were seen within a few seconds of the planet, which were not visible on November 5th; but then two others were to be traced, one of which could not have been overlooked in the first instance, had it been in the same position. If these were satellites, as is not improbable, there would be no *great* difficulty in taking good measurement both of their distance and position.

There could be little doubt of the high illuminating power of such a telescope, yet an example or two may be desirable. Between ϵ^1 and ϵ^2 Lyræ, there are two faint stars, which Sir J. Herschel (Phil. Trans. 1824) calls "debilissima," and which seem to have been, at that time, the only set visible in the twenty-foot reflector. These, at the altitude of 18°, were visible *without an eye-glass*, and also when the aperture was contracted to twelve inches. With an aperture of eighteen inches, power 600, they and two other stars (seen in Mr. Cooper's achromatic of 13·2 aperture, and the Armagh reflector of 15) are easily seen. With the whole aperture, a fifth is visible, which Dr. Robinson had not before noticed. Nov. 5th, strong moonlight.

In the nebula of Orion, the fifth star of the trapezium is easily seen with either speculum, even when the aperture is contracted to eighteen inches. The divided speculum will not show the sixth with the whole aperture, on account of that sort of disintegration of large stars already noticed, but does, in favourable moments, when contracted to eighteen inches. With the solid mirror and whole aperture, it stands out conspicuously under all the powers up to 1000, and even with eighteen inches is not likely to be overlooked.

Comparatively little attention was paid to nebulæ and clusters, from the moonlight, and the superior importance of ascertaining the telescope's defining power. Of the few examined were 13 Messier, in which the central mass of stars was more distinctly separated, and the stars themselves larger than had been anticipated; the great

nebula of Orion and that of Andromeda showed no appearance of resolution, but the small nebula near the latter is clearly resolvable. This is also the case with the ring-nebula of Lyra; indeed, Dr. Robinson thought it was resolved at its minor axis: the fainter nebulous matter which fills it is irregularly distributed, having several stripes or wisps in it; and there are four stars near it, besides the one figured by Sir John Herschel in his catalogue of nebulæ. It is also worthy of notice, that this nebula, instead of that regular outline which he has there given it, is fringed with appendages, branching out into the surrounding space, like those of 13 Messier, and in particular, having prolongations brighter than the others in the direction of the major axis, longer than the ring's breadth. A still greater difference is found in 1 Messier, described by Sir John Herschel as "a barely resolvable cluster," and drawn, fig. 81, with a fair elliptic boundary. This telescope, however, shows the stars as in his figure 89, and some more plainly, while the general outline, besides being irregular and fringed with appendages, has a deep bifurcation to the south.

From these and some other discrepancies, Dr. Robinson thinks it of great importance that the globular nebulæ and clusters should be all carefully reviewed, as it is chiefly from their supposed regularity that the hypothesis of the condensation of nebulous matter into suns and planets has arisen; an hypothesis which he thinks has, in some instances, been carried to an unwarrantable extent.

On the whole, he is of opinion that this is the most powerful telescope that has ever been constructed. So little has been published respecting the performance of Sir W. Herschel's forty-foot telescope, that it is not easy to institute a comparison with *that*, the only one that can fairly be made to compete with it. But there are two facts on record which lead to the inference that it was deficient in defining power: one, the low power used, which Dr. Robinson thinks was not above 370; the other, the circumstance that neither the fifth nor sixth stars of the trapezium of the nebula of Orion were shown by it. As to light, there is no reason to believe that the composition of the forty-foot mirror was as reflective as that of the twenty-foot; and if Dr. Robinson be correct in the opinion, that the latter* did not show the fifth star easily, or the sixth at all, and that it only exhibited the "debilissima" and one star near the ring-nebula, then *it* has decidedly less illuminating power than eighteen, perhaps not more than fourteen inches aperture of Lord Oxmantown's mirror, notwithstanding the loss of light in that by the reflexion at the second speculum.

However, any question about this optical pre-eminence is likely soon to be decided, for Lord Oxmantown is about to construct a telescope of unequalled dimensions. He intends it to be six feet aperture and fifty feet focus, mounted in the meridian, but with a range of about half an hour on each side of it. If he succeeds in giving it the same degree of perfection as that which he has attained in the present instance, which is exceedingly probable, it will be in-

* In its original state, not as improved by the more perfect means latterly employed by Sir John Herschel.

deed a proud achievement; his character is an assurance that it will be devoted, in the most unreserved manner, to the service of astronomy; while the energy that could accomplish such a triumph, and the liberality that has placed his discoveries in this difficult art within reach of all, may justly be reckoned among the highest distinctions of Ireland.

November 30.—Dr. Kane read a paper "On the Production of Audible Sounds," an abstract of which will be found in the present volume, p. 247.

The following note, "On the Course of the Diurnal Fluctuations of the Barometer," by James P. Espy, A.M., of Philadelphia, was communicated by Dr. Apjohn.

"It is a law of inertia, that if a body is forced upwards, it will react and press on its support more than its natural gravity; and if it is permitted to descend, it will press on its support less than its natural gravity, and the increase and diminution of pressure will be proportional to its velocity.

"Moreover, if a body is permitted to descend with a certain velocity, and then retarded, it will, when retarded, press more on its support than its natural gravity, and that in proportion to the rapidity of its retardation.

"This principle will explain the four fluctuations of the barometer which occur every day.

"Just before sunrise, when the atmosphere is neither becoming hotter nor colder, the barometer will indicate the natural weight of the air, which we may call a mean; as the sun rises the air will begin to expand by heat, and the whole atmosphere will be lifted up by this expansion, and by its reaction will cause the barometer to rise; and this will be the greatest at the time when the air is receiving the most rapid accessions of heat, which must take place before the hottest time of the day, when the air is becoming neither hotter nor colder. On this principle, then, the maximum day fluctuation will take place between daylight in the morning and the hottest time of the day, and this corresponds with the fact; for this maximum, which amounts to more than the tenth of an inch, takes place about nine or ten o'clock A.M.

"At the hottest part of the day, when the air is neither expanding nor contracting, it is manifest that the barometer will stand again at a mean. Soon after this, however, the air will begin to contract from diminishing temperature, and at the moment of the most rapid acceleration of contraction, the barometer will stand at its day minimum, which will probably be late in the afternoon; and it is found in fact to be from four to five o'clock. From this time the rapidity of the downward motion of the air from contraction begins to diminish, and the barometer of course begins to rise; and at the moment when it is most rapidly retarded in its contraction, the barometer will be at its maximum night fluctuation, and will again be above the mean, but not so much as the day maximum.

"This maximum is found to occur about ten or eleven o'clock P.M. The air will now go on contracting more and more slowly, until

about daylight, when it will be at rest, and the barometer will again be at a mean.

"This theory was given by me to the Journal of the Franklin Institute, and published ten or twelve years ago.

"I ventured in that paper to predict, notwithstanding some alleged observations at St. Bernard's Hospital to the contrary, that it would be found by more careful observations that the morning maximum fluctuation would be greater in lofty situations, on the sides of mountains, provided they were not very lofty, than on the plain below.

"For it is manifest, that there will be not only a reaction at these lofty situations, (a little less, it is true, than below,) but some of the air will be lifted up, by the expansion of the air below, above the upper place of observation; which would in all probability more than compensate the diminished reaction at moderate elevations.

"This prediction has been entirely verified by Lieutenant-Colonel Sykes's observations in India, and this verification may be considered as a strong proof of the correctness of the theory. It is quite probable, that maximum day fluctuation occurs later at considerable elevations than on the plain below.

"The theory would lead us also to suppose, that at very *great elevations*, where the reaction is very minute, only two fluctuations would be found in the day: the maximum at about two o'clock P.M., when most air would be above the barometer; and the minimum at daylight in the morning, when least air would be above it; but I know of no observations to confirm or refute these deductions."

Mr. Ball brought under the notice of the Academy the fact, that the ordinary Sturgeon of the Dublin markets is an undescribed species. He stated that Mr. Thompson of Belfast, and Professor Agassiz, concurred with him in this opinion, and he proposed to call it *Accipenser Thompsoni*, purposing, if permitted, to give figures and full descriptions in a future number of the 'Proceedings.'

The Archbishop of Dublin made some observations on a remarkable meteor, lately seen in different parts of Britain.

December 14.—Dr. Apjohn read the following notice, by George J. Knox, Esq., of "some Improvements in the Voltaic Pile."

"The chief imperfection in the voltaic pile—its want of a constant uniform power of long duration, by which it is rendered almost useless as an instrument of research—having been overcome by the ability of Professor Daniell, the only thing that remained to render it efficient seemed to be, to increase its power; a desideratum accomplished by Mr. Grove, by substituting for copper and sulphate of copper, platinum and nitric acid.

"The following experiments were undertaken with the intention of estimating the relative values of the different constructions of Grove's battery, recommended by Mr. Knight of Foster Lane, as far as respects the arrangement of the zinc and platina plates, when, to my surprise, I found the *same* quantity of electricity to be evolved when the zinc is bent so as to expose an opposing surface to *each* surface of a platinum plate, as when a platinum plate, of the size of the former zinc, is similarly placed with respect to a plate of zinc of

the same size as the former platinum, affording an œconomical method of arranging a Wollaston's battery, the zincs being bent round the coppers, in place of the coppers round the zincs.

"*Experiments with Grove's Battery.*—The acid solutions were those recommended by Mr. Grove, i. e. pure nitric acid, in contact with the platina; sulphuric acid + 4·5 water by measure, in contact with the zinc. The surfaces of zincs immersed were 3 by 2·5 inches; those of the platina, bent round the porous vessel holding the zincs, were 6 by 2·5 inches. The glasses containing the acid, &c., were 3·2 inches long, 1·5 broad, 3·5 deep. The length of the porous vessel of pipeclay was 2·5 inches, the breadth 0·3, the depth 3·5. The number of alternations was five.

	Cubic inches.
Time, 2 minutes	8·0
The battery being at rest for 10 minutes	8·0
... ... 25 ...	3·0
... ... 1 hour	1·0
... ... 19 hours	none

"The porous vessel was found filled with sulphate of zinc, which stopped the action of the battery.

"*Second Experiment.*—The zinc plates being of the same size as the former platina, and the platina of the same size as the former zincs; the zincs bent round the platina; all other things being as before.

	Cubic inches.
Time, 2 minutes	8·
After 10 minutes	8·
... 25 ...	8·

"*Third Experiment.*—Another battery, the diameter of the cells of which was $2\frac{1}{2}$ inches, gave a diminution of only *one-half* of the quantity of gas after the lapse of *forty-eight hours*, showing the advantage of having a large supply of sulphuric acid*.

"*Experiments with Smee's Battery of Platinized Silver.*—The acid solution was of the same strength as before, and the sizes of the zincs and platinized silver the same as of the zincs and platina formerly employed. The zincs were bent round the platina†.

	Cubic inches.
Time, 2 minutes	3·
After 5 minutes	2·6

"*Second Experiment.*—The zincs being raised out of the acid, cut in two, and re-immersed.

* "The porous vessels were of pipeclay. The same experiments repeated with unglazed porcelain gave 10 cubic inches in two minutes; with very porous pipeclay, they gave as much as 15 cubic inches in two minutes, showing the importance of attending to the nature of the porous vessel employed."

† "The most advantageous method of arranging a Smee's battery is, packing the zincs and platinized silvers in the manner recommended by Dr. Faraday in his tenth series, (also by Mr. Young, Phil. Mag., S. 3. vol. x. p. 241) placing the package on supports so as to allow the sulphate of zinc to fall to the bottom of the vessel, while the fresh acid rises to the surface."

	Cubic inches.
Time, 2 minutes	1·6
After 5 minutes	1·6

"*Third Experiment.*—The zincs and platinized silver being removed, the acid remaining untouched; the platinized silver plates were bent round the zincs.

	Cubic inches.
Time, 2 minutes	2·6
After 5 minutes	2·4

"*Fourth Experiment.*—The platinized silver cut in two.

	Cubic inches.
Time, 2 minutes	1·4
After 5 minutes	1·4

"Supposing, from these experiments, the same quantity of electricity to be developed, whichever of the opposed surfaces of the two metals be the greater, I placed in separate glasses five zinc cylinders, one-inch diameter, immerged eight-tenths of an inch in the acid; platina foil, connected by binding screws with the zincs, was rolled into cylinders two-tenths of an inch in diameter, and then immersed in pipeclay tubes one inch deep.

	Cubic inches.
Time, 2 minutes	1·0
After 10 minutes	1·0

"*Second Experiment.*—Platina foil of the same size as the zincs, and zinc rods of the same diameter as the platina cylinders being employed, the effects were precisely the same.

	Cubic inches.
Time, 2 minutes	1·0
After 10 minutes	1·0

"*Third Experiment.*—The zinc cylinders being made twice the diameter of the former; the quantity of gas generated in two minutes was the same as before; the increased number of lines of electrical force compensating the increased resistance offered by the acid solution.

"*Fourth Experiment.*—With cylinders twice the diameter of these, a very feeble current passed, the obstacle being too great to be overcome; by increasing the diameter of the porous vessels, and thereby of the nitric acid solution, which is a good conductor, the impediment is diminished, as shown in experiment fifth. Thus Mr. Binks (Phil. Mag., S. 3. vol. xi. p. 68) finds, that in dilute sulphuric acid, the size of the copper compared to a given surface of zinc, to produce a maximum effect, should be 16, that of the zinc to a given surface of copper being 7; while in a galvanic arrangement, in which the zinc is immersed in dilute sulphuric acid enclosed in a membranous bag, and the copper in a surrounding solution of sulphate of copper, the proportion of zinc to copper was as one to eight, the impediment to the passage of the current being double in the latter case what it was in the former*.

* "The experiments of Prof. Daniell have proved, that in the constant battery, when the generating surface formed the circumference of the arrangement, the force was only the half of that evolved when it formed the centre."—Phil. Trans., vol. cxxviii. p. 41.

"*Fifth Experiment.*—Five cylinders of zinc, 10 inches high, $\frac{1}{2}$ diameter, were placed in glass vessels containing sulphuric acid, as before. Into these were placed cylindrical earthenware vessels, $1\frac{1}{2}$ inch diameter, containing pure nitric acid; slips of platina foil were rolled into cylinders as before.

	Cubic inches.
Time, 2 minutes	20·0
After 10	25·0
After 30	30·0*

"From these data may be calculated the heights of the zinc pipes, and the weight of platina foil required to obtain any given decomposition, to be employed, as shown by Jacobi, either as a motive power, or applied to light-houses, to the polariscope, or to the fusion of refractory substances. For the latter purposes, I had fixed to a strong shallow woolf-bottle, two tubes with glass cocks, and to them tubes containing chloride of calcium, applied to a Daniell's jet, playing upon a cylinder of lime rotated by clockwork. A third tube was inserted in the bottle, intended as a regulation of the pressure, or a safety-valve, in case of explosion."

Dr. Apjohn then made a brief verbal communication on the subject of the composition of Pyrope. This mineral, long confounded with garnet, is known to be distinguished from it by containing chrome, and by exhibiting, not the dodecahedral, but the hexahedral form. The best analyses of it, however, which are by Kobel and Wachtmiester, are obviously imperfect, of which no better proof can be given than that Gustavus Rose, in his Crystallography, does not attempt to give the formula of the mineral, but contents himself with enumerating the different oxides of which it is composed. Under these circumstances, Dr. Apjohn conceived that a re-examination of the constitution of pyrope would not be without interest. He therefore undertook its analysis; and the result has been that he has detected in it yttria, one of the rarest of the earths; one, in fact, which had previously been known to exist only in a few minerals of exceeding scarcity. The yttria was insulated in the following manner:—

The mineral being fused with carbonate of potash, and the silex separated in the usual way, the peroxide of iron, alumina, and yttria were precipitated together by a mixed solution of ammonia and salammoniac. The alumina was taken up by caustic potash; and to the iron and yttria, dissolved in a minimum of muriatic acid, such a quantity of tartaric acid was added, that upon subsequently pouring in ammonia in excess there was no precipitate produced. The iron was now removed by sulphuretted hydrogen; and the solution evaporated to dryness, and ignited in a large platinum

* "The dilute acid in the voltameter began to boil; the cause of the increase of decomposition, compared to what took place in the small cylinder, was the small stratum of sulphuric acid between the porous vessel and the zinc. For a continuous action, the zinc pipes, sealed at one end and amalgamated, should be connected, by pipes at top and bottom, with an earthenware vessel containing the sulphuric acid."

crucible, so as to volatilize the ammoniacal salts and burn away the carbon of the tartaric acid, left the *yttria* slightly coloured by oxide of chrome. From this latter substance it is separated, but not perfectly, by the action of a dilute acid; and by the addition of ammonia, or caustic potash, to the solution, the yttria is again recovered. That the substance thus obtained is yttria, seems proved by the following considerations:—

It is separated, though not completely, from acids by ammonia largely diluted with sal-ammoniac, and hence cannot be one of the alkaline earths.

It is insoluble in potash, and is, therefore, not alumina or glucina.

After ignition it dissolves readily in dilute acids, and is hence not zirconia or thorina. From zirconia it is further distinguished by its saline solutions being precipitated by ferrocyanide of potassium.

It is not oxide of cerium, for it does not redden in the exterior flame of the blow-pipe, and because its salts are not precipitated by the sulphate of potash. The quantity of the yttria amounts to at least 3 per cent.

Dr. Apjohn is still engaged in investigating the composition of pyrope; and expressed his intention of bringing his results on a future occasion in a more detailed form under the notice of the Academy, when he hoped also to be able to assign the true formula of the mineral.

Mr. Clibborn made the following communication on the subject of the Leyden Jar:—

"In Brande's Manual of Chemistry, vol. i., 3rd edition, p. 76, I find it stated, that 'if one Leyden jar be insulated, with its internal surface connected with the positive conductor, another jar may be charged from its exterior coating; and if this second jar be insulated, a third may be charged from its exterior coating, and so on for any number of jars, provided always that the exterior coating of the last jar be connected with the ground.'

"As my electrifying machine was but small, it occurred to me that I might œconomize both time and labour by constructing a battery of jars so arranged that I should be able to take advantage of this principle, and make one jar charge another, instead of my being obliged to charge the whole series; for, though they are all connected together, and charged by the same operation in the common electric battery, yet the time and labour consumed in charging the battery is exactly the same as if each jar were charged separately and then added to the series. A great saving of labour and time would have been effected had the arrangement of jars answered, for it was exactly the same as that described by Brande, so far as the charging part of the apparatus was concerned; but when the jars were loaded, or rather *should have been loaded,* they were made to turn through a quadrant, and form a new arrangement, by which all their outside coatings were connected together by a common conductor. A similar arrangement connected all their inside coatings, which made all the conditions necessary to the perfection of the common battery; and I found it capable of being charged by the electrifying machine in this form, but it could not be charged to any extent in

the other. It appeared that but few sparks would pass from the conductor to the first jar. If the last one was removed, and its chain fastened to the next, the first jar would take a few more sparks, and so on; for it was found, that whenever the last jar in the series at any time was removed, the same results followed; and this was the case when the last but one was removed, clearly proving that the capacity or aptitude of the first jar to take a charge was influenced and diminished by the second, more so by the third, fourth, &c. Its aptitude was greatest when it was by itself, and not connected, as described, with the others.

"This result disappointed my expectations, so far as my intended improvement on the electric battery was concerned; and it also appeared to point out the existence of a principle influencing the charge of the electric jar, which was not recognized in the popular treatises on electricity. I procured a number of glass plates with fixed and moveable coatings. These plates were insulated and arranged with and without coatings in every way that Brande's rule required, but the general result was the same as that given above.

"From numerous experiments made with these plates, I came to the following general conclusions:—

"1. That the actual quantity of the positive and negative electricities which we can accumulate in the *opposite surfaces* of an electric or non-conductor, as a plate of glass or dry ice, depends upon the *distance* of these surfaces.

"2. Every case of charge of *one* jar or plate may be assimilated to that of any number of jars or plates in a series such as Brande's, by supposing the one jar or plate to be divided into the greater number, its thickness being the *sum of the thicknesses of all the segments or plates*; the inside of the first jar or surface of first plate in contact with conductor, and outside of last jar or plate in contact with the ground, being considered as the proper *opposite surfaces* of the proper plate, and those on which the electricities evolved by the friction of the cylinder and rubber of the electrifying machine are accumulated or heaped.

"If we make a pile of the plates, coated or not, and charge the outside surfaces by coating them, and connecting one with the cylinder and the other with the rubber of the machine, we find all the conditions of the experiment complied with. There is no necessity for any connexion with the ground, which in Brande's can act merely as the conductor to convey the negative charge of the rubber to the extreme surface.

"Let us now unpack the pile, and we find that the charge of the intermediate plates diminishes as we approximate towards the centre of the pile, being greatest near the extremes. At equal distances the charges are equal; for the charges of the first plate but one, and the last but one, will as perfectly neutralize each other as the charges of the surfaces of the first and last. The same is found to be the case with the surfaces of the third plates from each extreme, and so on of the others; but it is not the case with a second and a third, a first and a fourth plate, and so on, *no two unequals as to place* exactly

neutralizing each other. Hence we may conclude, that the charge of the intermediate jars in a series such as described by Brande, though it really depends on inductive agency, is altogether different from that kind he alludes to, which may be inferred from his erroneous representation of the actual fact; and the charge of the extreme surfaces is immediately the result of that action only, which several electricians have called conduction, arising from the connexion of these surfaces with the sources of the free electric forces.

"The fact here described appears capable of throwing much light on the corpuscular arrangement of the atoms of bodies which retain an electric charge on their surfaces, or which, by a change of form from mechanical pressure or difference of temperature, exhibit differences of electric state. In speaking of a charged electric, we may consider it a pile of an infinite number of plates, each of which, except the extreme surfaces, is composed of a surface of atoms, which are acted on by two sets of induced electric forces, whose differences, arising from their distances from the extremes, we discover when we split the plate, or if it be a pile, when we separate the plates from each other."

LXXIX. *Intelligence and Miscellaneous Articles.*

DR. MARTIN BARRY ON THE CORPUSCLES OF THE BLOOD.

DR. BARRY requests us to add the following, in connexion with his paper on the Corpuscles of the Blood, an abstract of which is given at p. 517.

It is known that in the blood there are found "colourless globules" along with the red blood-discs. Dr. Barry has noticed that in certain states these "globules," though pale, are not without colour. Of such "globules" he has figured a considerable number in the paper in question, and cannot distinguish them from bodies described by him in that paper as noticed in the heart, on the one hand, and at the edge of the fœtal crystalline lens, on the other; in both of which situations the bodies in question are what he denominates *parent* corpuscles of the blood. At the edge of the lens, such bodies are represented by him as seen discharging discs; which having acquired red colouring matter, are no other than young blood-discs. These discs accumulate in such quantity as to completely *fill* certain dilated and varicose vessels, situated at the edge of the lens. The discs begin to exhibit pellucid, colourless, and nearly fluid centres. These centres enlarge, and in the same proportion the surrounding red colouring matter is consumed; so that the varicose vessels contain little besides the enlarged and colourless centres of the discs. This substance, *derived from red blood-discs*, it appears to be that, exuding, enters into the formation of the lens; and now, even in the elements of the crystalline lens, red colouring matter is reproduced. Dr. Barry is unable to assign a mode of origin different from that just mentioned, to the corpuscles he has traced into other

structures, or to the "exudation-corpuscles" of authors: and, as he shows, the floating red blood-discs have their origin in parent cells.

COMPOSITION OF MELLITIC ACID.

M. F. Wœhler obtains this acid by decomposing the mellitate of lead or of silver; the first by hydrosulphuric and the last by hydrochloric acid, any excess of the latter being separated by evaporation. Mellitic acid is very soluble, and crystallizes from a concentrated solution in small reticulated crystalline needles of a silky lustre. This acid suffers no change by exposure to the air; it has a strongly acid taste, fuses when heated, and burns in the air with a brilliant sooty flame, with an aromatic smell, and yielding a great quantity of carbon, without leaving any residue. When heated in a retort a portion is volatilized without decomposition, but the larger proportion is completely decomposed. Crystallized mellitic acid does not yield water till heated to nearly 400° Fahr.

It is composed of nearly—

Carbon	42·38	or 4	equivalents.
Oxygen....	41·21	.. 3	....
Water	16·41	.. 1	....
	100·		

L'Institut, No. 383.

SALTS OF LEAD CONTAINING ACIDS OF OXYGEN AND AZOTE.

M. Peligot has re-examined the salts of lead which contain the acids formed by oxygen and azote. He observes that Proust, about thirty years since, first obtained the yellow salt of lead by dissolving metallic lead in solution of the nitrate. He was of opinion that the salt contained a suboxide of lead. It was however shown by Berzelius, that the lead was dissolved, not by reducing the oxide of lead to a lower state of oxidizement, but at the expense of the nitric acid.

The subnitrite which contains the least of oxide of lead, was found both by Berzelius and Chevreul to consist of

Acid and water	20	
Oxide	80	
		100

The subnitrite containing most lead was found to consist of

	Berzelius.	Chevreul.
Acid	10·175	9·9
Oxide	89·825	90·1
	100·	100·

M. Chevreul showed, however, that notwithstanding this agreement in the analyses, the nitrites which he had prepared were different from those of Berzelius. Thus the latter found no water in the nitrite containing most oxide, while Chevreul did. Berzelius says, that this salt crystallizes in small scales of a brick-red colour; while that obtained by Chevreul was in scales of a flesh-colour, &c.

In experimenting on these compounds, M. Peligot generally agreed

with Berzelius and Chevreul as to the oxide of lead; but he is of opinion that these salts, of which he finds there are three, contain nitrous acid, formed, according to the experiments of Dulong, of two volumes of azote and 4 volumes of oxygen, and is prepared by the decomposition of nitrate of lead.

The salt discovered by Proust is prepared by dissolving an equivalent of neutral nitrate of lead, 166, in about fifteen times its weight of water, and putting into it an equivalent of lead, 104, in very thin plates, when heated to about 140° to 155° F. If more lead be used the orange salt is formed, if less, dinitrate of lead is obtained with the yellow nitrite; if the heat be not greater than above stated, no nitric oxide gas is formed.

The results of the analyses of this salt are given by M. Peligot as under:—

Az	177·04	5·0
O^4	400·00	11·7
2 Pb O	2789·00	80·1
H O	112·50	3·2
	3478·54	100·

Converting these into English equivalents, this salt is composed of nearly—

One equivalent of	nitrous acid	46	16·5
Two ...	protoxide of lead	224	80·2
One ...	water..............	9	3·3
		279	100·

The next salt examined by M. Peligot was the orange-red compound. It is prepared by boiling one equivalent of nitrate of lead with one and a half of lead. The composition of this salt is stated to be—

Az^2	354·0	3·1
O^8	800·0	7·3
7 Pb O	9761·5	86·7
3 H O	337·5	2·9
	11253·	100·

English equivalents will give

Two equivalents of	nitrous acid	92	10·2
Seven ...	protoxide of lead ..	784	86·9
Three ...	water	27	2·9
		903	100·

This salt may also be procured by boiling a solution of the yellow nitrite with protoxide of lead.

The last salt analysed by M. Peligot is very sparingly soluble in water, 10,000 parts at common temperatures dissolving only eight parts, which are taken up by 290 parts of boiling water. In order to form this salt, the lead must be boiled in very dilute solutions of the nitrate. When the water holding it in solution is quickly cooled, this salt precipitates in the state of a white powder, whereas, by slowly cooling, silky crystals of a light rose-red are obtained, resembling carbazotate of ammonia.

This salt, according to M. Peligot, consists of

Az	177·0	2·8
O^3	300·0	5·0
4 Pb O	5578·0	90·4
HO	112·5	1·8
	6167·5	100·

and he calls it a basic nitrite of lead, as determined by Berzelius and Chevreul.

Considered as a sub-hyponitrite of lead, its composition will be nearly—

One equivalent of hyponitrous acid..	38	7·7
Four equivalents of protoxide of lead	448	90·5
One equivalent of water	9	1·8
	495	100·

Ann. de Chim. et de Phys., Mai 1841.

INQUIRY RESPECTING A MS. AT OXFORD, REFERRED TO BY MR. RUTHERFORD IN HIS PAPER ON THE RATIO OF THE DIAMETER OF A CIRCLE TO ITS CIRCUMFERENCE. BY J. O. HALLIWELL, ESQ., M.A., F.R.S.

To the Editors of the Philosophical Magazine.

GENTLEMEN,

In an interesting paper by Mr. Rutherford on the ratio of the diameter of a circle to its circumference, just printed in the Phil. Trans.*, a reference is made to a manuscript at Oxford, in which the numerical approximation is carried as far as 152 places of decimals, but no press-mark is given to the manuscript; and, after a careful search through all the printed catalogues, I have not been able to find an account of any manuscript at all likely to contain such a calculation. There are very few scientific manuscripts in the Bodleian Library of a later date than the *fifteenth* century, and it is quite impossible that the approximation could have been carried so far at that early period. If Mr. Rutherford possesses a reference to the manuscript in question, or any clue to it, he would confer an obligation on me, and I have little doubt on many others, if he would kindly communicate it to your Magazine, and it is very probable the manuscript may contain other matters worthy of notice. At all events, it would be satisfactory to ascertain the *date* of the manuscript.

I am, Gentlemen, your obedient servant,

Dec. 17th. J. O. HALLIWELL.

* [Of which an abstract will be found at p. 561 of the preceding volume. —EDIT.]

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